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Fig. 1.



Fig. 2.

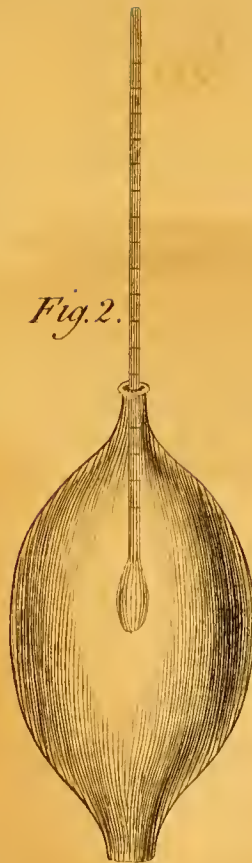
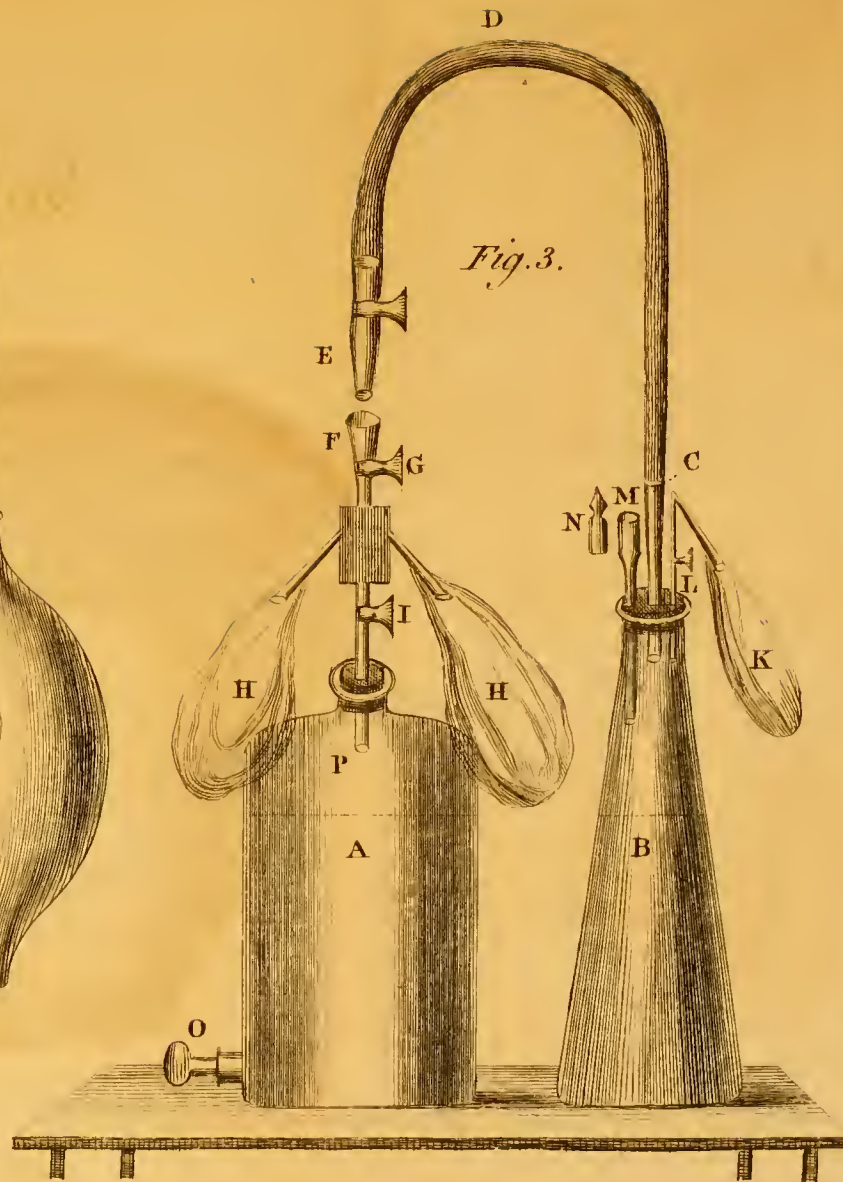


Fig. 3.



No. 2 m

EXPERIMENTS
AND
OBSERVATIONS
RELATING TO VARIOUS BRANCHES OF
NATURAL PHILOSOPHY;
WITH
A CONTINUATION
OF
THE OBSERVATIONS ON AIR.

The SECOND VOLUME.

By JOSEPH PRIESTLEY, LL. D. F. R. S.

Honorary Member of the Academy of Sciences at *Petersburg*,
and of the Royal Academy of Medicine at *Paris*.

Vires acquirit eundo.

VIRGIL.

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ПРЕДМЕТ

или

ОБЪЕКТ

исследования

НАУЧНО-ИССЛЕДОВАТЕЛЬСКИЙ

ЦЕНТР

ЭКОНОМИКИ И

УПРАВЛЕНИЯ

ТЕОРЕТИЧЕСКОЕ ИССЛЕДОВАНИЕ

В ОБЛАСТИ

ЭКОНОМИЧЕСКОГО

РАЗВИТИЯ И
УПРАВЛЕНИЯ

ИТОГИ

ИССЛЕДОВАНИЙ

В ОБЛАСТИ
ЭКОНОМИКИ И
УПРАВЛЕНИЯ

T O

WILLIAM HEBERDEN, M. D.

AN ENCOURAGER OF EVERY PURSUIT
THAT IS INTERESTING TO MEN, PHI-
LOSOPHERS, AND CHRISTIANS,

THIS VOLUME

I S,

WITH THE GREATEST RESPECT,

INSCRIBED, BY

HIS MOST OBLIGED

HUMBLE SERVANT,

J. PRIESTLEY.

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1910-1911

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1910-1911

1910-1911

1910-1911

1910-1911

T H E
P R E F A C E.

THE progress I have made in my philosophical pursuits since my last publication will not, I flatter myself, disappoint the expectations of my reader, provided they be reasonable. This volume, like all the preceding ones, contains a mixture of things of greater and of less importance, both with respect to the doctrine of *air*, and to subjects which have usually been referred to the general head of chemistry.

He will find that I have prosecuted with considerable success the experiments on the *green vegetable matter*, of which I treated in my last volume; nothing having been then wanting, but the certainty of this being a *vegetable substance*, to my advancing one considerable step in tracing the manner in which

this and other growing vegetables purify noxious air, viz. by means of the action of *light* upon them; having then sufficiently satisfied myself, that it was *light* only, and not *heat*, that enabled this substance, whatever it was, to yield pure air. And from this I have been led to determine other important circumstances relating to the general œconomy of nature. The reader will also find other new facts relating to the several kinds of air, which give new views of their constitution.

On this account I had intended to have drawn out a *general theory* of all the kinds of air, and *tables of affinity*, to explain the various phænomena in which they are concerned. But having many things yet to ascertain before I can satisfy myself with respect to this subject, and being unwilling to obtrude any crude conceptions on my readers, I have thought proper to defer it till I shall have made some farther progress in my inquiries.

However,

However, the summary and methodical view that I have given of all the *facts* that I have discovered will assist any person to form as good a theory as I could for him. For all that is properly meant by *a theory*, exclusive of *hypothesis*, is a number of general propositions, comprehending all the particular ones, deduced from single experiments. A general theory will also be the less necessary on account of my having made all the use I could of the most important new facts, in observing, at the time that I mention the discoveries, how much they seem to affect our ideas of the constitution of the particular kinds of air to which they relate.

This volume would probably have been larger, or have made its appearance earlier, but that the former part of the last summer was engrossed by a tedious and dangerous illness, and the latter part of it was taken up by my removal from Calne to Birmingham. But my health being, by the goodness of God, in a good measure re-established, I flatter myself I shall be able to devote myself as

much as in any former period of my life to philosophical pursuits; and during my incapacity for making experiments, my hints for the farther prosecution of them are greatly accumulated. But whether my application to these studies will be crowned with *success* is a thing for which I cannot answer. I can only promise to follow with assiduity such lights as have occurred, or that may occur to me.

It has been one consequence of the late interruption of my philosophical pursuits, that many courses of experiments recited in this volume are left imperfect. But whenever the publication had been made, this must, from the nature of the thing, have been, in a great measure, unavoidable. No philosophical investigation can be said to be completed, that leaves any thing unknown that we are prompted by it to wish we could know relating to it. But such is the necessary connection of all things in the system of nature, that every discovery brings to our view many things of which we had no intimation

mation before, the complete discovery of which we cannot ~~but~~ help wishing for; and whenever these discoveries are completed, we may assure ourselves they will farther increase this kind of dissatisfaction.

The greater is the circle of light, the greater is the boundary of the darkness by which it is confined. But, notwithstanding this, the more light we get, the more thankful we ought to be, for by this means we have the greater range for satisfactory contemplation. In time the bounds of light will be still farther extended; and from the infinity of the divine nature, and the divine works, we may promise ourselves an endless progress in our investigation of them: a prospect truly sublime and glorious. The works of the greatest and most successful philosophers are, on this account, open to our complaints of their being imperfect. Did Dr. Hales excite no wish that he did not satisfy with respect to *air*? And did Newton himself solve every query concerning *light*?

I am

I am ready, however, to acknowledge, that more experiments (which the sagacious reader will see I might have made) would probably have carried me considerably farther in several of the subjects of inquiry. But having materials enow for a volume, and entering, as I may say, upon *a new period of life*, I was willing to close my philosophical accounts as they stand at present, before I open a new one. And whenever I have made any publication, I have been unwilling to with-hold from my reader any thing concerning which I was able to give him even imperfect information. By this means he is fully possessed of all that I myself know on the subject, and is therefore equally qualified to satisfy his own impatience, in whatever respects I may have left it unsatisfied. He cannot but see, if he considers what is now before him, that many new and promising fields of inquiry are open to us. And I thank God I am now in a situation in which I have every advantage for entering upon them.

Having

Having sufficient room for it, I have introduced into this volume the paper on the *lateral explosion*, from the Philosophical Transactions of the year 1760. By this means every thing that I have written on philosophical subjects will be found in these five volumes, or in my *History of Electricity*. There only remains in the Philosophical Transactions, a few papers of mine containing general results of processes, the details of which will be found in this work.

I have also addressed a *Letter to Mr. Kirwan* on his Notes to *Mr. Scheele's treatise on air and fire*. But it contains little more than will be found at large in this volume, except an opinion which I have long entertained, that the explosive power of *aurum fulminans* is owing to the setting at liberty a great quantity of alkaline air, an opinion which I have not yet verified by experiment, but of which I entertain little doubt.

The method I have used in this volume is the same with that of the preceding, viz.
analytic

analytic and *historical*, but as concise as possible. Details of many experiments will always be tedious to those who look for nothing but general results; but to those who chuse to prosecute the inquiries themselves, they will be found absolutely necessary. I shall myself be obliged to look back to all the circumstances that I have noted, whenever I resume the investigations; and I therefore conclude they will be of the same use to others. My wish is not only to gratify the generality of those persons who take pleasure in reading philosophical writings, but also to assist future experimenters. Others may content themselves with seeing the conclusions, and pass by the details.

It has necessarily happened, from my having published my experiments as soon as the historical account of them would supply materials for a volume (a conduct with which I see every reason to be satisfied) not only that some things required to be prosecuted farther, but that others have required *correction*. On this account, all those who now read the
whole

whole work from the beginning, will wish to be informed, as they proceed, how far the articles under their immediate consideration have received illustration from the farther prosecution of the same subjects, of which an account is given in the subsequent volumes.

For this reason I conceived that it must be useful, in this advanced state of the work, to revise the whole from the beginning, and write notes on those passages on which I can throw more light than I was able to do at the time when they were written. Such notes I have now drawn up. They relate chiefly to the *first volume*. In future time, myself, or others, may be able to throw as much light on the imperfect observations in the later volumes.

It has also happened from the publication of this work at different periods, that the arrangement of the whole, as *one work*, could not be regular; some things having been discovered last, which, in an orderly system, ought to have been placed first; and many things

things nearly allied in nature being widely distant from each other; a defect which no *Index*, in the usual form, can sufficiently remedy.

For this reason I have also thought it would be useful (and indeed I have been frequently urged to it by my friends) to draw up a *methodical index*, or a *summary view of all the more important facts*, with references to those parts of the work in which they are more largely treated of. This I have now done with some care, but in as few words as possible, the most general results only being recited, referring to the work itself for all details and processes. A few things, however, are repeated, when they equally came under two different heads, and were of sufficient importance. Such a summary view as this will be particularly useful to suggest what is yet most wanted to extend this branch of knowledge, and to direct the views of future experimenters.

I cannot

I cannot conclude this Preface without expressing my joy that the progress of philosophy does not seem to be much retarded by the calamities of war, in which a great part of Europe is unhappily involved; except that the correspondence of philosophers in distant parts of the world is interrupted by it, and a greater difficulty is found in procuring foreign publications.

The principal subject of this volume is pursued with great ardour by many foreigners, as well as by philosophers in this country; and since the character that Virgil gives to *Fame* is still more applicable to *Philosophy*, *vires acquirit eundo* (which I have taken for a motto to this volume) we may flatter ourselves with a prospect of a rapid progress in discoveries of the most interesting nature.

I also cannot help expressing a wish that with the return of peace (and happily it is not in the power of any state to be always at war) we may see every obstruction to the progress of knowledge, which is equally
friendly

friendly to all states, removed. Taxes on the importation of books, and other articles of literature, are so impolitic, as well as illiberal, that it is earnestly wished that something may be stipulated by the contending powers for abolishing them. There are statesmen whose minds are sufficiently enlarged to see that Philosophy gives an ample equivalent for the exemption.

Birmingham, 24th March, 1781.

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T H E

I N T R O D U C T I O N.

HAVING appropriated the *Introduction* of each of the former volumes to the description of apparatus, and an account of improvements in the method of making experiments, and having done little of this kind since my last publication, an Introduction to this volume was hardly necessary. It may be worth while, however, to give a short account of the *earthen jar* in which I made many of the experiments on the growth of plants in different kinds of air, recited in this volume; and a bare inspection of *fig. 1*, will be almost sufficient for this purpose.

The jar was about eighteen inches in diameter at the top, and of the same depth.

It was placed in an open exposure in the garden, and sticks were thrust into the earth in a perpendicular position, quite round it; and to these sticks glass jars, filled with water, with their mouths inverted in the water of the earthen jar, were fastened by strings. After I had introduced into one of these jars any particular kind of air, I afterwards drew through the water, and put into it, any plant, the top and leaves of which I wished to expose to it; supporting the root or stalk at a proper height in the earthen jar, if I found that any such support was necessary. In some cases it will be found that the top of the plant was in one jar, and the root or stalk in another; which it was not at all difficult to do.

Fig. 2, represents the instrument by which I endeavoured to ascertain the conducting power of different kinds of air with respect to heat. It consists of a glass bulb open at both ends, so that I could easily fasten a thermometer with its bulb in the center of it, where it would be surrounded by any kind
of

of air, introduced into it after it had been previously filled with mercury. The manner in which the experiments were made is sufficiently described in the account of them, p. 375.

I have made several improvements in my method of making experiments, but they are not of importance enough to deserve a particular description. If I should make any farther alterations, I may perhaps, on some future occasion, give a drawing and description of my whole apparatus, according to the latest improvements that I shall be able to make in it.

It may be proper in this place to remind my reader that, in measuring the purity of respirable air, I mix with it an equal quantity of nitrous air, or if it be highly dephlogisticated, two equal quantities of nitrous air, which is always particularly mentioned in the course of the volume: after this I transfer the mixture into a graduated tube. Consequently a less number in the result is always an indication of greater purity. This number, in order to be as
concise

XX THE INTRODUCTION.

concise as possible, I have in this volume termed *the measure of the test*. Thus if when I mix two equal quantities of common air and nitrous air, they afterwards occupy the space of one measure, and two tenths of a measure, I say *the measures of the test were 1. 2.*

O B S E R V A T I O N S
RELATING TO
VARIOUS BRANCHES
OF
NATURAL PHILOSOPHY.

S E C T I O N I.

*Of the growth of the WILLOW PLANT in
different kinds of air.*

I N my last publication I observed that the willow plant grew very well both in inflammable and in common air, and that it absorbed a considerable proportion of both the kinds, as well as of nitrous air. In this there could not possibly be any mistake, unless we suppose the water to have absorbed the air, which it has never been known to do in any similar circumstances. However, when I resumed the experiments on the growth of this plant in the course of the

last summer, I had no instance of the absorption of common air; but I had repeated, and very extraordinary ones of the absorption of inflammable air by it, and the plant flourished so remarkably in this air, that it may be said to feed upon it with great avidity. This process terminates in the change of what remains of the inflammable air into phlogisticated air, and sometimes into a species of air as good as common air, or even better; so that it must be the *inflammable principle* in the air that the plant takes, converting it, no doubt, into its proper nourishment.

Some other plants also, as *Comfrey* and *duck-weed*, I observed to thrive very well in inflammable air, and to produce a similar effect upon it, though, as I observed in my first publication on the subject of air, and upon other occasions since, *mint* does not thrive so well in this as in common air, and I have generally found that, in time, this plant is killed by it.

It may deserve to be mentioned in this connexion, that the willow plant grows best
in

in marshy places, which abound with inflammable air. The plants that I made use of grew in the bottom of a field, in and near a piece of water, into which, if I only thrust a stick, a prodigious quantity of inflammable air rushed out, so that, without changing my place, I could, at any time, collect a large receiver full of it; and bubbles of air were very frequently rising spontaneously from the mud at the bottom. It may, therefore, be a provision in nature, that this noxious kind of air should be fitted to the nourishment of such plants as grow best in those places in which it abounds, as well as that plants in general should purify the common atmosphere.

The facts from which these conclusions are drawn, as well as some farther observations on the subject, are the following, in the recital of which it will be necessary to mention the month and the day on which the observations were made, as they have a connexion with the state of the plant, and probably with its powers of action on air.

On the 26th of May 1779, I put a jar of about twenty ounce measures of air over a willow plant growing in water, and on the first of June I observed that the air was little diminished in quantity, or affected in quality; for by the test of nitrous air the measures were 1.33; that is, when one measure of nitrous air was mixed with one measure of this air, they occupied the space of 1.33 measures. The plant continuing to grow, I examined it on the 5th of June, when the measures were 1.3, and those of the common air at the same time, I observed, were 1.26. This slight degree of injury I imputed to some black leaves, which were then about the plant. On the 8th of the same month, the measures were 1.36, and on the 15th they were 1.4; and there was still no more prospect of the air being absorbed than before. This I thought very extraordinary, as in the preceding summer I had always found, take what care I could, that these plants injured common air, and at least diminished it in the usual degree of one

one fourth, if they did not absorb more of it.

In inflammable air the results were consistent with the preceding observations, and uniform with themselves. But the year before I had no opportunity of pursuing these observations to the extent that I wished, so that I could not tell in what state the plants would finally leave the air; whereas now I had sufficient time fully to satisfy my curiosity in this respect.

On the 18th of May I introduced one of these plants, growing in water, under a jar of strong inflammable air, and the 3d of June following, I found that it was diminished about one third. Examining it, I found it was but weakly inflammable. This plant had not room to expand itself, but still it lived very well. On the 31st of the same month, there was no more than one third of the air remaining in the jar, and it was still slightly inflammable. Owing to some accident or other, the plant had been dead about a week, after which time I

observed that the air had ceased to be diminished.

I then introduced another plant into what remained of the air, and on the 5th of June it was reduced one third more, and then I could not perceive that there was any thing inflammable in it. It was also a good deal dephlogisticated; for with two equal quantities of nitrous air, the measures of the test were 1.6, so that, upon the whole, the growth of this plant in this kind of air had the same effect upon it as agitation in water would have had, viz. diminishing it, depriving it of its inflammability, and rendering it in some measure respirable.

I had another result exactly corresponding with this. For on the 9th of June I examined a quantity of inflammable air, in which a willow plant had grown from the 26th of May; but in this case not more than about half the quantity was absorbed, but part of the remainder fired with one explosion, like a mixture of common and inflammable air; and applying the test of nitrous

trous air, the measures were 1.43, which is about that state of air in which a candle just goes out. On the 15th of June another quantity of this air, in which a willow plant had grown from the same time, was fired in the same manner, and the measures of the test were 1.44; though only about one half of it had disappeared.

Another quantity of the same kind of inflammable air, in which a willow plant had grown the same time, was reduced to one sixth of its original quantity. It then exploded like a mixture of common and inflammable air, and the measures of the test were 1.53. It was examined on the 9th of June. On the 15th of the same month it was diminished still more, and had then nothing inflammable in it, but the purity was nearly the same; the measures of the test being 1.54.

In all the experiments that I made of this kind, the quantity of air absorbed was very various, depending probably, upon the health of the plant, its size in proportion to that of the jar, and other circumstances.

On the 24th of May I had introduced one of these plants into a jar of inflammable air, collected from the marsh near which I had gathered it; and on the 9th of June I found it so far diminished, that little more than one seventh of the original quantity remained. This was merely phlogisticated air; for it was not affected by nitrous air, and extinguished a candle.

On the 15th of June, I found that another quantity of the same kind of inflammable air, in which a willow plant had grown from the same date, was not diminished near so much; for about one third of the original quantity was left. This, however, was partly inflammable, the slightest blue flame imaginable being perceived in a large jar of it. When I applied the test of nitrous air, the measures were 1.62.

A sign of the great vigour of the plants growing in inflammable air, was the vivid greenness not only of the leaves that were in the air, but of those also that were under water, and the length of time that they continued so in these circumstances; whereas, in
general,

general, when the top of the plant was in common air, the leaves that were under water soon became discoloured, and perished. These leaves on the contrary, not only continued green, but were always loaded with air bubbles, which were continually detaching themselves, and rising into the jar, having their places supplied by others. These bubbles, I had no doubt, consisted of the air that had been strained, as it were, through the plant, leaving its phlogiston behind, for the nourishment of the plant. I endeavoured to collect a quantity of these bubbles, before they mixed with and diluted the inflammable air in the top of the jar, but I did not succeed. I have no doubt but that it would have been dephlogisticated air, as this will easily account for the state in which I found this air in the experiments recited above.

It was doubtful, however, whether these bubbles consisted of air that had been imbibed by the leaves, and then passed through a considerable space within the substance of the plant, or of the air that had been contained

tained in the water, to which these leaves had immediate access. The latter seems more probable from some experiments, but the following are nearly decisive in favour of the other supposition.

I put the stalk of a willow plant into an inverted jar full of water, while the top of it was in a jar of inflammable air. In these circumstances a small quantity of air was collected in the inverted jar, and it was evidently better than common air. This air I had observed to come from all the outside of the stalk, but especially from the places where the leaves had been broken off; and there were some few bubbles from the middle of the place where the stalk itself had been cut, for it had no root.

In another experiment of this kind, when the plant had been in the situation above described, from the 11th to the 14th of June, three fourths of an ounce measure of air was collected in the inverted jar, so pure, that the measures of the test were 0.63; and with two measures of nitrous air 1.5. Applying the flame of a candle at the orifice of a tube
filled

filled with it, there was a loud explosion, so that it was a mixture of dephlogisticated and inflammable air.

On the 19th of June, I collected half an ounce measure more from the same plant; and applying to it the test of nitrous air, the measures were 0.9, and there was nothing sensibly inflammable in it. Had there been nothing inflammable in the air collected in the inverted jar, containing the stalk of the plant, the probability would have been, that all the air came from the water, dephlogisticated by the action of the plant; but the mixture of inflammable air in it seems to prove that part of it, at least, had been imbibed by, and strained through the plant, entering at the leaves (which alone were exposed to the inflammable air) and issuing at the stalk, which was turned up into the other jar in which the air was received. This singular case, for it is the only result I ever had of the kind, shows that the plant had taken in more nourishment than it could properly digest.

This plant thriving so remarkably well in inflammable air, and depriving it of its inflammability,

flammability, I thought it could not well fail to purify phlogisticated air, if I gave proper attention to its health and ease in its confined situation, though (perhaps through want of this attention) it had failed to do so the preceding summer; and I was not disappointed in my expectations at this time.

On the 22d of June I introduced one of these plants into a jar of air phlogisticated by the putrefaction of fishes, confined by rain-water, in which I had found by frequent trials, that the green vegetable matter was not soon generated; and on the 26th of the same month, it was so much improved, that the measures of the test were 1. 38, which is a little better than the state in which air will just extinguish a candle. The 3d of July, I examined it again, and then the measures were 1. 32, and on the 15th of the same month, it was exactly of the standard of common air. The water by which it was confined certainly produced no air; for another jar filled with water, in the same trough, and therefore having precisely the same exposure with respect to light, and all other circumstances,

circumstances, had no air at all in it. A very little air was strained through this plant, and it was almost thoroughly phlogisticated; for the measures of the test were 1. 7.

Nitrous air I have always found to be fatal to vegetable, as well as to animal life, and so it proved in this instance; as indeed it had done the preceding summer. From the 18th of May to the 18th of June, a quantity of this air was diminished by a willow plant to one fourth, and then it was so changed, that it admitted a candle to burn in it with a gently blue enlarged flame; a state which, as I have observed, nitrous air generally passes through before it becomes mere phlogisticated air, and which, in the course of this volume, will appear to be nitrous air partially dephlogisticated.

Phlogiston being the pabulum of plants, as it is probably of animals too, dephlogisticated air must (as indeed I had found before) be unfavourable to the growth of plants in general; and I constantly found it to be so in the case of the willow plant. To give it the fairer trial, I introduced a small but healthy

thy plant, growing in the marsh, into a jar of this air, so large that the plant was not in the least incommoded, and it did not reach the top of the jar by several inches. This was done the 18th of May, but it died presently, and before the air was sensibly diminished; which was the case afterwards, owing, probably, to the putrefaction of the plant. But even then, being examined with two equal quantities of nitrous air, the measures of the test were 1. 0.

Having filled a large earthen pot with water, and having sticks thrust into the earth quite round it, for the convenience of fastening jars with their mouths inverted in water, in order to fill them with different kinds of air, and introduce plants into them, without the trouble of going to the marsh in which they grew; I filled one of these jars with dephlogisticated air, and then introduced the top of a willow plant into it. In a day or two, all the part that was within the jar began to turn white, and was soon after manifestly quite dead, even when many shoots of the same plant that were under water continued

tinued green, and looked well a considerable time afterwards. The air, being examined, was found to be very little injured. I therefore think we may safely conclude, that dephlogisticated air is universally hurtful to plants; and this, *a priori*, would be an argument in favour of the depuration of atmospherical air by vegetation.

Having made the preceding experiments on inflammable air with the willow plant, I proceeded to try a few other plants; and without giving such particular attention to these as to those of the willow plant, I soon found that *comfrey*, which is hairy like the willow plant, and grows best in the same situation, and also the *meadow sweet* grew very well in inflammable air. So also did *duckweed*, which was always remarkably healthy, and of a deep green colour, a certain sign, I believe, of health and vigour in plants in general; whereas, in dephlogisticated air, duckweed always presently became pale, and died.

SECTION II.

Of the purification of air by plants and the influence of light on that process.

ONE of my earliest observations on the subject of air, but made casually, when, in fact, I expected a contrary result from the process, was the purification of air injured by respiration or putrefaction, by the vegetation of plants. But at that time I was altogether ignorant of the part that *light* had to act in the business. At the publication of my last volume, I had fully ascertained the influence of light in the production of dephlogisticated air in water by means of a *green substance*, which I at first supposed to be a plant, but not being able to discover the form of one, I contented myself with calling it simply *green matter*.

Several of my friends, however, better skilled in botany than myself, never entertained any doubt of its being a plant; and I had afterwards the fullest conviction that it must be one. Mr. Bewly has lately observed

served the regular form of it by a microscope. My own eyes having always been weak, I have, as much as possible, avoided the use of a microscope.

The principal reason that made me question whether this green matter was a plant, besides my not being able to discover the form of it, was its being produced, as I then thought, in a phial close stopped. But this being only with a common cork, the seeds of this plant, which must float invisibly in the air, might have insinuated themselves through some unperceived fracture in it; or the seeds might have been contained in the water previous to its being put into the phial. Both Mr. Bewly and myself found, in the course of the last summer, that when distilled water was exposed to the sun, in phials filled in part with quicksilver, and in part with distilled water, and inverted in basons of quicksilver, none of this green matter was ever produced; no seed of this plant having been able to penetrate through the mercury, to reach the water incumbent upon it, though, in several cases, it will be

seen, that these seeds diffuse and insinuate themselves, in a manner that is truly wonderful.

Without light, it is well known, that no plant can thrive ; and if it do grow at all in the dark, it is always white, and is, in all other respects, in a weak and sickly state. Healthy plants are probably in a state similar to that of *sleep* in the absence of light, and do not resume their proper functions, but by the influence of light, and especially the action of the rays of the sun. This was the reason why no green matter was ever produced by means of mere *warmth* in my former experiments, and that in jars standing in the same exposure, but covered so that the light had no access to them, no pure air was collected, none of the green matter being then found in them.

This I verified most completely by covering the greatest part of a glass jar with black sealing-wax, which made it thoroughly opaque ; and besides answering that purpose better than brown paper, as I made the experiment mentioned in my last volume

lume, did not imbibe any of the water, and therefore did not promote the evaporation of it. To be able to observe whether any air was collected in these jars, or not, the upper part of them was not coated with sealing-wax, but had a thick moveable cap of paper, which I could easily take off, and then inspect the surface of the water.

In order to satisfy myself as fully as possible with respect to this remarkable circumstance, I also made the following experiments, the result of which are, indeed, very decisive in favour of the influence of *light* in this case.

Having a large trough of water, full of recent green matter, giving air very copiously, so that all the surface of it was covered with froth, and jars filled with it, and inverted, collected great quantities of it, and very fast; I filled a jar with it, and, inverting it in a basin of the same, I placed it in a dark room. From that instant no more air was yielded by it, and in a few days it had a very offensive smell, the green vege-

table matter with which it abounded being then all dead, and putrid.

Again, having filled a receiver with fresh pump water, and having waited till it was in a state of giving air copiously, I removed it into a dark room; and from that time the production of air from it intirely ceased. When I placed it again in the sun, it gave no air till about ten days after, when it had more green matter, the former plants being probably all dead; and no air could be produced till new ones were formed.

With the same view I placed some small slices of *roasted beef* in a vessel of water in the sun, and an equal quantity, in another vessel of the same size, in the dark; when the former became green, and yielded air, but the latter not at all. It will be seen afterwards that many animal substances afford an excellent pabulum for this green vegetable matter.

I also made a similar experiment with slices of *cucumber*, when those in the sun became covered with green matter, and yielded pure air, but those that had been placed in the shade,

shade, though they did yield a small quantity of air, it was wholly phlogisticated, though not inflammable, which many vegetable substances yield in the same circumstances.

That it was the *green matter*, that yielded the air, and not the mere action of *light* upon the water, might be inferred from my former experiments; and this was my own idea at the first, though I quitted it afterwards. The appearance which then misled me was the great quantity of pure air emitted by the water, after it was poured off from the green matter. But before any air can appear on the surface of the water, in its elastic state, the water itself must be thoroughly saturated with it, in which case it contains so much air, that, upon the least agitation, even without heat, it readily parts with it, and exhibits the beautiful appearance which I then described. But that, notwithstanding this appearance, it was the *green matter*, and not the *water* that yielded the air, I was convinced by the following experiment.

Having a number of earthen plates covered with green matter, I introduced several

ral of them under vessels filled with fresh pump water, and then placed them in the sun, together with other vessels filled with the same water, at the same time, but standing on clean plates; when I constantly found that air was immediately produced in the vessels containing the green matter but none in the others, till the green matter was naturally formed in them; after which, but not before, pure air was produced in those vessels also.

I likewise used water that had long been exposed to the sun's light, so that it must have deposited every thing that mere *light* could make it part with; and yet in this water, upon plates of green matter, air was immediately produced, as well as in water that had never been exposed to the sun.

I was led to these experiments by observing that air was immediately produced from those parts of my jars to which green matter from former experiments happened to adhere, not having been carefully cleaned. It was likewise an evidence that it was the green matter, and also in a vegetating state, that

that yielded the air, that when a plate covered with it had been made pretty hot before the fire (by which the plants had probably been killed) it was incapable of yielding any air.

Having, by this means, fully satisfied myself, that the pure air I had procured was not from the *water*, but from the green vegetating substance assisted by light, I concluded that other *aquatic plants* must have the same effect; and going to a piece of stagnant water, the bottom of which was covered with such plants, I took five or six different kinds promiscuously. Then having put them into separate jars of the water in which they were growing, and inverted them in basons of the same, I placed them in the sun; and I found that all of them, without exception, were immediately covered with bubbles of air, which gradually detaching themselves from the leaves and stalks, where they had originated, rose to the surface of the water; and this air, being examined, appeared to be, in all the cases, very pure, though not quite so pure as that

which I had before procured from the green matter; the measures of the test, with two equal quantities of nitrous air, being, at a medium, 1.5. Afterwards air procured from these plants was very nearly as pure as the other.

In order to ascertain with more precision the real origin of this pure air, and especially to determine whether it was properly *produced* by the light, and something within the plant (which, as I found afterwards, seems to be the idea of Dr. Ingenhoufz *) or only

* He says (Experiments on vegetables p. 23) that “ the
 “ air obtained from the leaves is by no means air from
 “ the water, but air continuing to be produced by a spe-
 “ cial operation carried on in a living leaf, exposed to
 “ the day light, and forming bubbles, because the sur-
 “ rounding water prevents this air from being diffused
 “ through the atmosphere.” Again, p. 89, he says of
 the green vegetable matter, “ It is wonderful that this
 “ green matter seems never to be exhausted of yielding
 “ dephlogisticated air, though it has no free communica-
 “ tion with the common atmosphere, from which the
 “ most part of other plants seem to derive their stock of
 “ air. Does this vegetable matter imbibe this air from
 “ the water, and change it into dephlogisticated air?
 “ this does not seem to me probable---I should rather in-
 “ cline to believe that the wonderful power of nature,
 “ of changing one substance into another, and of promot-
 by

by dephlogisticating the air, previously contained in the water, which I suspected from my former experiments on vegetation, I kept a quantity of these water plants in jars of water in the sun, as long as they would give any air; then only changing the water. I found that the same plants immediately began to give fresh air as copiously as at first. The particulars of the experiment were as follows.

I put a handful of these water plants, without distinguishing their kinds, into a receiver containing eighty ounce measures of water, inverted in a basin of the same; and when they had yielded between six and seven ounce measures of air, I examined it, and found that with two equal quantities of nitrous air, the measures of the test were 0.8. But the air had been diminishing about three days, so that I believe there had been eight ounce measures in all, or one tenth of

“ ing perpetually that transmutation of substances which
“ we may observe every where, is carried in this green
“ vegetable matter in a more ample and conspicu-
“ ous way.”

the

the capacity of the jar, and certainly purer than it was now found to be. It was evident, therefore, that no more air would have been produced by these plants in this water, though placed in the sun. Replacing this jar with more of the same river water, the same plants were instantly covered with air bubbles, and in a very few hours had yielded more than an ounce measure of air. Some *duck weed*, which swam on the top of the water, in the former part of this experiment, was dead, owing, no doubt, to the purity of the air to which it had been exposed.

To conclude this series of experiments, I expelled air from a quantity of this river water, both before the plants were put into it, and afterwards; and I found that the air contained in it was purer after the plants had been confined in it than before, though the whole piece of water being quite full of plants, the air contained in it was tolerably pure in the first instance. The measures of the test, with an equal quantity of nitrous air being 1. But the air expelled from the
water

water after the plants would grow in it no longer, was so pure, that with *two* equal quantities of nitrous air, the measures of the test were 1. Also, whereas the phial of water in the first case gave only 2.4 ounce measures of air, the same phial afterwards gave 4.4; which is nearly twice as much. For, as I have observed before, whereas the phlogistification of air diminishes the quantity of it, the dephlogistification must increase the quantity; and this increase exceeding the quantity which the water is capable of holding in solution, part of it is detached, and appears in an elastic form on the surface.

It is also a proof that the proper origin of all the air produced in these circumstances is not the plant and the light, and that these are only agents to produce that effect upon something else, that in all cases, the quantity of air produced bears a certain general proportion to the capacity of the vessel in which the process is made, never, I believe exceeding one eighth, exclusive of that
which

which is held in solution by the water itself, which, however, is pretty considerable.

A jar containing one hundred and fifteen ounce measures was filled with pump water the 2d of June, and it presently began to yield air, and continued to do so about a fortnight, after which very little was produced. The quantity I received from it was twelve ounce measures, which is more than one tenth of the bulk of the water, and as highly dephlogisticated as almost any that I had ever procured. The reason why this jar began to yield air immediately was, that green matter from former experiments adhered to several parts of it.

At another time I observed, that when a large earthen trough, filled with pump water, was very turbid, with green matter floating in it, and in a state of giving air very plentifully, if I inverted any jar full of it, it would, in about a week, yield one eighth of its contents of air; and examining it, I found this air so pure, that with two equal quantities of nitrous air, the measures of the test were 0.5;
and

and it is not often that dephlogisticated air is obtained more pure than this.

From this experiment it was very evident, that there is no proper *production* of air in the case, but only a *depuration* or dephlogistication of the air previously contained in the water; and as water plants depurate the air that is held in solution by the water, it is agreeable to analogy that plants growing in air should depurate that air to which they are exposed. This led me to try whether plants growing in air would, when wholly immersed in water, though it be not their natural element, exert, and retain for any time, their power of depurating air. But still, to keep as near as I could to the water plants, with which I had had so much success, I pitched upon the *water flag* for the experiment; the root of this plant and part of the stalk being in water; though the upper part emerges out of it. Not suspecting that the mere *leaves* of a plant retain so much life as Dr. Ingenhousz found them to do, (and as I might have learned from Mr. Bonnet) I took three whole plants, and put them into tall
jars

jars of water for the purpose; when I observed that the leaves were presently covered with air bubbles, and continued to give air the whole day. This air I observed to stream from both sides of the leaf, and every part of the stalk. This air I examined, and found it, in one case, to be something worse than common air, but in another case, something better, though not considerably so. Before I proceeded to make trial of any other plants, I was informed of the experiments of Dr. Ingenhoufz, whose assiduous attention to this subject gave me the greatest satisfaction, and entirely superseded what I might otherwise have thought of doing in the same way.

It appears from these experiments, that air combined with water is liable to be phlogisticated by respiration, and to be dephlogisticated by vegetation, as much as air in an elastic state, out of water. For fishes, as I have observed, foul the air contained in the water in which they are confined, and water plants now appear to purify it. This is no doubt one of the great uses of weeds, and other aquatic plants, with which fresh
water

water lakes, and even seas abound, as well as their serving for food to a great number of fishes.

The experiments recited in this section may help us to explain why water, after issuing from the earth, and employed in floating meadow land, becomes in time exhausted of its power of fertilizing them. When it issues from the earth, it contains air of an impure kind; that is, air loaded with phlogiston. This principle the roots of the grass extract from it; so that it is then replete with dephlogisticated air, and consequently the plants it afterwards comes into contact with, find nothing it in to feed upon.—I believe it is commonly imagined, that the water deposits something in its course upon the earth of its bed, and by that means becomes effete, and incapable of nourishing plants.

SECTION III.

Farther observations on the GREEN VEGETABLE MATTER with which many experiments in the preceding volume were made.

I Very much doubt whether the green matter, which had been the subject of the preceding experiments, has ever been properly noticed by botanists. The *conserva fontinalis*, as it is described by Dr. Withering, in his most useful system of botany, though said to have threads *extremely short*, is only said to have them "Sometimes not more than half an inch in length," and it is also said to be of a *brownish green*. Whereas this whole plant cannot be one tenth of an inch in length, and it is of a beautiful lively green. It will be thought, however, I imagine, to come most properly under the denomination of the *conserva*; but this not being within my province I shall not presume to give it any particular appellation, though

though I might be inclined to call it *the water moss*. I shall therefore continue to call it, in general, the *green matter*, or the *green vegetable substance*. Whether this plant has ever got a name in systems of botany, or not, its *natural history* is certainly unknown; and therefore, in this and the following section, I shall give such an account of its mode of growth, and other particulars relating to it, as have happened to fall under my observation*.

* Dr. Ingenhoufz's idea of the origin of this vegetable matter, as he himself allows it to be, is rather extraordinary, considering how long the doctrine of *equivocal*, or *spontaneous generation*, has been exploded. He says, p. 90, "The water itself, or some substance in the water, is, as I think, changed into this vegetation, and undergoes, by the influence of the sun shining upon it, in this very substance, or kind of plants, such a metamorphosis, as to become what we now call dephlogisticated air. — This real transmutation, though wonderful in the eye of a philosopher, yet is no more extraordinary than the change of grass and other vegetables into fat, in the body of a graminivorous animal, and the production of oil from the watery juice of an olive tree." But the change of *water*, into an *organized plant*, is a thing of a very different nature from these.

In general the feeds of this plant (for I presume that, like other plants, this also must have feeds) float invifibly in the air, and are capable of producing plants in all feafons of the year, whenever they meet with water, efpecially if it be impregnated with not too great a proportion of vegetable or animal fubftance, in a ftate of putrefaction; and if it does not actually freeze, the plants never fail to appear in their vigour, fo as to be capable of producing air, in the fpace of a few days. But though the richeft pabulum for this plant is the putrefcent parts of animal and vegetable fubftances, fome of them are unfavourable to it, and prevent its growth.

The feeds of this plant infinuate themfelves into veffels of water through the fmalleft apertures, and then diffufe themfelves through the whole mafs of it; fo that when the largeft jars are filled with water, and placed inverted in bafons of the fame, and confequently the feeds muft enter between the bafons and the bottoms of the jars, the plants will firft appear at the very
top

top of the jar, if the best pabulum for it be lodged there. It will likewise appear from the following experiments, that, though the tendency to produce pure air is favoured by a certain quantity of putrifiactive matter in the water in which these plants are found, the quantity may be so great, as to counteract the operation of the plant, and phlogistificate and diminish the air as fast as it is produced.

As I shall generally describe the whole of every process, just as I noted the appearances at the time, the necessary *influence of light* in the production of dephlogisticated air, as well as other circumstances already proved by the experiments recited above, will occasionally receive additional confirmation.

I have found a flower and a less produce of air from rain water than from pump water; owing, I suppose, to the rain water containing less air to operate upon, and generally also in a purer state than that which is contained in pump water.

On the 8th of June, I placed, in the open air, a large jar of rain water, inverted in a

bason of the same; but no green matter appeared in it before the 22d of the month. On the 24th of July, finding no more air produced, I examined it, and found the quantity to be two ounce measures and a half: and with two equal quantities of nitrous air, the measures of the test were 1.24. This rain water, which was received in a large tub from the roof of a house, yielding so little air of itself, I generally made use of it when I tried the effect of different im-pregnations of water.

The green matter, and consequently the production of air, also generally appeared very late in *distilled water*, which also is a confirmation of the hypothesis mentioned above. For water after distillation must have time to imbibe air from the common atmosphere for this plant to operate upon, before any air can be produced from it. On this account, I have always found that this effect has been soonest produced in the smallest vessels. Having, on the 20th of August exposed to the air a jar nine inches deep, and another of four inches, not in-
verted

verted in basons, but simply filled with distilled water, the latter was covered with green matter on the 6th of September; whereas it did not appear in the taller vessel till a considerable time afterwards.

In one experiment (from which, however, I would not draw any certain conclusion) distilled water was more favourable to the production of this green matter than rain water, which, being collected from the roof of a house, might contain some peculiar impregnation unfavourable to vegetation.

I put 4dwt. of boiled mutton into the belly of a retort, containing about a pint, filled with distilled water, and an equal quantity of the same mutton I put into a retort of the same size, filled with rain water; and observing them nine days afterwards, I found the latter of a reddish hue, with very little air, whereas the former was all green, and in a state of yielding air very copiously. The mouth of this retort was immersed in a vessel of water seven inches deep, and was also closed with

a cork, which had a very small perforation in it, in order to cut off, as much as possible, all communication with the external air. Perhaps the seeds of this plant might have been in this water previous to its exposure, though it had been distilled not long before the experiment.

I found this green matter in a state of giving air in water, which had formerly been impregnated with fixed air and iron. The fixed air, however, being gone, and the iron precipitated, nothing but simple water was left. But I likewise found this plant in water impregnated both with *common salt*, and with *saltpetre*, which impregnations water will not part with in the open air.

The water was impregnated with common salt, so as to make it of about the same degree of saltiness with that of sea water, and it was exposed in a tube an inch in diameter, and three feet long, inverted in a pot of the same water. All the inside of the tube was in time nearly covered with small green knobs, almost contiguous to each other, and not with such an uniform coating

ing as is generally found in common water. The air was very pure. Dephlogistified air was also procured at the same time in a similar tube, filled in the same manner, with water impregnated with an equal quantity of *nitre*. But the air in this appeared not to be quite so pure as that in the water impregnated with common salt.

Having impregnated a quantity of water very strongly with fixed air, I placed it in an inverted phial, and observed that no green matter appeared in it of a long time; but when the fixed air might be supposed to have made its escape, the green matter appeared; and the air, when examined, was found to be of the purest kind, without the least mixture of fixed air in it. With two equal quantities of nitrous air, the measures of the test were 0.5.

In order to observe on what part of a vessel of water the seeds of this plant would first fall, and in what manner they would then propagate themselves, I placed in the sun a glass tube one inch wide and three feet long, in an inclined position, but with

its mouth upwards, filled with distilled water. The green matter first appeared, in small specks, about two inches below the surface of the water, on the side to which it was inclined, then on the same side near the middle of the tube, and lastly all the bottom was covered with it. On the whole, the tube presented the appearance of the seeds having been let fall into it perpendicularly, and passing through the water to have fixed themselves where they happened to impinge. Had the tube been placed perpendicularly, the green matter would, I suppose, have appeared first at the bottom of it, as indeed I have generally found to be the case, and would have extended itself from thence to the sides of the tube.

SECTION IV.

Of the production of green matter, and of pure air, by means of various VEGETABLE SUBSTANCES in water.

HAVING very soon observed that this green vegetable matter, or *water moss*, was planted and propagated with more ease, and produced air more copiously, in some circumstances than in others, and that various substances, animal and vegetable, were favourable to it, and others of both kinds unfavourable, I tried a great variety of them, and shall recite such of the particulars as appear in any measure remarkable, and such as may furnish hints for the farther investigation of what relates to this subject.

The most remarkable circumstance attending these experiments was, that some substances, concerning which I could have had no such expectation a priori, instead of admitting the growth of this plant, when they began to putrify, and dissolve, which was the case with most vegetable and animal substances,

substances, yielded from themselves a very great quantity of inflammable air; and it made no difference whether they were placed in the sun or in the shade. Whereas other substances, which, if they had been confined by quick-silver, would have yielded, by putrefaction, inflammable air also, together with a portion of fixed air, only supplied the proper pabulum for this green matter, and the whole produce was pure dephlogisticated air; the phlogiston, which in other circumstances would have been converted into inflammable air, now going to the nourishment of this plant, which by the influence of light yields such pure air.

I shall, in the first place, give an account of the experiments I made with the *leaves* of plants, and then with some other parts of them, confining myself chiefly to such as are commonly used for food; having in that choice a view to the *principle of nutrition*, besides that such substances were most at hand.

On the 18th of June, I put 18 dwts. of green *cabbage* into a large jar of rain water.

On

On the 28th the water began to be a little turbid, and the vessel contained three ounce measures of air, no part of which was fixed air, and, with two equal quantities of nitrous air, the measures of the test were 1.44. Having changed the water, and left the cabbage in the same vessel, on the 18th of July there was in it six ounce measures of air, which was increasing very rapidly, all the water being very green; but after the 19th, little more air was produced. At this time I collected ten ounce measures, no part of which was fixed air, and with two equal quantities of nitrous air, the measures of the test were 0.67. The cabbage was then soft, but not offensive.

Replacing the same cabbage in fresh water, on the 27th of July several ounce measures of air were produced, and on the 29th I took from it eight ounce measures, the production of air having ceased a day or two before. This air was quite as pure as the last; for the measures of the test were 0.6. and the cabbage was still soft, but not in the least offensive. The reason of this, I imagine, was,
that

that the phlogiston, which would have constituted the offensive smell of the cabbage (and no putrid vegetable substance is more offensive) was, in this case, imbibed by this *water moss*, as fast as it was produced by the process of putrefaction; and the vessel being large, there was no superabundant phlogiston to contaminate the air.

In order to try what effect a larger quantity of cabbage in proportion to the size of the jar would have, and also what would be the difference of its putrefying in the *dark*, I made the following experiment.

On the 19th of July I put $2\frac{1}{2}$ ounce measures of cabbage into a small vessel of water in the sun, and in a similar vessel an equal quantity of the same cabbage in a dark room. On the 25th the water of the vessel in the sun had a whitish appearance, and about an ounce measure of air was produced; but at the same time there was a much larger quantity of air produced from the cabbage in the dark, the water being turbid also. The day following I examined the air from the dark room, and found it to
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be sixteen ounce measures, one third fixed air, and the rest strongly inflammable. The cabbage was putrid and highly offensive. That in the sun had yielded an ounce measure and an half of air, a very small proportion of which, perhaps one twentieth, was fixed air, and the rest slightly inflammable, the cabbage offensive.

This experiment shews that without light inflammable air is produced by the putrefaction of vegetable substances, and accounts for the production of this kind of air in marshes. The reason why the cabbage in the sun also produced inflammable air (though it was not in so great a quantity as from the cabbage in the dark) was that the mass of it was too great for the capacity of the vessel. There had also been very little sunshine, the weather having been rainy, or cloudy.

On the 28th of June I put fourteen dwt. of *lettuce* into a jar containing twenty ounce measures of rain water. On the third of July it became turbid, and two ounce measures of air were produced, the slightest proportion

portion of which was fixed air, and the rest strongly inflammable. The lettuce had a very offensive smell. In this case, as in the former, the quantity of lettuce, as I imagined, was too great for the production of pure air.

A branch of garden *spurge* put into a jar of rain water, the 28th of June, had yielded but a few bubbles of air on the 17th of July, neither fixed air nor inflammable, but of the standard of common air. I then replaced the spurge in a quantity of fresh water, and on the 27th of July I took from it an ounce measure and an half of air, so pure that, with two equal quantities of nitrous air, the measures of the test were 0.66; and it would probably have yielded more air. At the time of the first observation I imagined the plant was not sufficiently putrid.

The green vegetable matter upon this plant was of a peculiar species, quite different from any thing that I had ever observed before, or have seen since. One of the berries of the spurge was quite covered with
it,

it, and exhibited the appearance of such a figure as is generally drawn to represent the atmosphere of a comet. It consisted of filaments as fine as a hair, each of them about half an inch in length, rising perpendicularly from the surface of the berry. This beautiful appearance was first noticed by my friend Mr. Scholefield, who had favoured me with a visit that summer. This was probably the proper *conserva fontinalis*.

The next experiment exhibits very clearly the difference between the effect of *light*, and *no light*, with respect to the object of this inquiry. On the 30th of July I placed half a *cucumber*, weighing 15 dwt. in a vessel containing seventy ounce measures of water in the sun; and on the 24th of August I took from it one ounce measure of air, so pure that, with two equal quantities of nitrous air, the measures of the test were 1.0, not in the least inflammable, and without any mixture of fixed air. The cucumber was quite covered with the green vegetable matter, and had no bad smell.

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At the same time the other half of the same cucumber, which had been kept in a vessel of the same size in the *dark*, had yielded one third of an ounce measure of air, all of which was phlogisticated, and the cucumber was very offensive. In this case I doubt not that the air in its nascent state, as it may be called, was inflammable air, but had been changed into phlogisticated air, as inflammable air is very apt to be; in which case the quantity is always greatly diminished. Of this I shall produce several instances in the course of this volume.

The only *flowers* I made trial of were *white lillies*. Of these, on the 28th of June, I put 3 dwt. into a jar containing about forty ounce measures of rain water; and at one time during the process they seemed to have yielded about an ounce measure of air; but on the 17th of July the quantity was manifestly diminished, and when examined it appeared to be without any mixture of fixed air, and very nearly phlogisticated, the measures of the test being

ing 1.7. The lillies had no bad smell. I doubt not but the phlogiston, which is always exhaled in great quantities from flowers, had contributed to diminish and phlogistificate the better air that had been first produced, though there had been but little or no appearance of green matter in this vessel.

Potatoes I found to afford an excellent pabulum for this vegetable matter, and consequently to be exceedingly favourable to the production of pure air, but seemingly not at all when they are boiled.

On the 24th of July, a potatoe, weighing 2 oz. 2 dwt. 12 grains, cut into thin slices, was put into a jar containing a hundred and fifteen ounces of rain water, and placed in the sun. In a day or two the water became turbid, and air began to be emitted, the potatoe being quite covered with the green matter; and on the 28th all the water in the vessel was so full of green matter floating in it, that nothing could be seen in the inside of it. At the same time a low jar, containing about six ounces of water, with

a small potatoe, not sliced, had nearly the same appearance.

Afterwards, on the 3d of July, I put some slices of potatoe into a tall jar containing six ounce measures of water fresh distilled, having a communication with the water in the basin in which it was inverted by a glass tube, with a very fine orifice in the cork with which the jar was closed. About the 20th of August I observed these slices of potatoe to be a little green, and on the 24th they were wholly so, the green matter first appearing in the basin in which the jar stood, which was supplied from time to time with rain water.

In order to try what quantity of air I could procure by means of these potatoes, which appeared to be so well adapted to the purpose, I put three of them, each about the bigness of a small walnut, into a vessel containing 35 ounces of rain water. They yielded five ounce measures of air, so pure that, with two equal quantities of nitrous air, the measures of the test were 0.54. The potatoes were quite soft, but could not be
said

said to be offensive. Again, from a sliced potatoe weighing 2 oz. 2 dwt. 12 grains, exposed to the sun from the 24th of July, in a jar containing 115 ounces of rain water, I took, on the 6th of August, ten ounce measures of air, the measures of the test, with two equal quantities of nitrous air, being 0.58, the potatoes quite soft as those above.

Lastly, from 15 dwt. of *boiled potatoes*, which had been exposed in the sun a long time, in a small receiver, I took about half an ounce measure of air, a small proportion of which was fixed air, and the rest phlogisticated air. This potatoe was never green. What would have been the result if the quantity of water had been greater, I cannot tell.

From three slices of *turnip*, exposed to the sun in a vessel containing ninety ounces of water, I took nine ounce measures of air, so pure that, with two equal quantities of nitrous air, the measures of the test were 0.75.

Nothing I ever tried was, in general, more unfavourable to the production of

pure air than *onions*. It was only by using a very small quantity of them, and by exposing them to the sun in a very large quantity of water, that I succeeded to make it admit the green matter. At length, however, from $5\frac{1}{2}$ dwt. of onion, exposed to the sun in a jar containing 200 ounces of water, from the 6th of August to the 31st, I got six ounce measures of air, not in the least inflammable, and so pure that, with two equal quantities of nitrous air, the measures of test were 1.2. At the same time I had exposed 13 dwt. 23 grains of the same onions in a jar containing $3\frac{1}{2}$ ounces of water, and on the 9th of October following I took of it a little more than half an ounce measure of air, all of which was phlogisticated. It extinguished a candle, and was not at all affected by nitrous air. There had been twice as much air in the vessel a month or six weeks before, and then it was probably inflammable.

S E C T I O N V.

Of the production of air by means of the green matter from ANIMAL substances.

ANIMAL substances were not, upon the whole, more favourable to the growth of this green vegetable matter, and the production of pure air from it, than vegetables; and different kinds of animal substances exhibited as great differences in this respect.

One of the first and most remarkable appearances that I had of this kind occurred in some experiments that I was making with *fishes*. It shews how readily the seeds of this aquatic vegetable find their proper pabulum, notwithstanding a great mass of water be in their way to it.

On the 13th of June I put three very small fishes into a jar containing 200 ounces of rain water, inverted in a basin of the same, when there was presently a thin filmy substance peeled off from all the surface of the fishes. After this a red matter, I sup-

pose dissolved blood, issued from them, and was diffused through the whole mass of water, making it very turbid. About the 23d of June the red matter became, as it were, green, the green vegetable substance adhering to it; and on the 26th the whole mass of water was exceedingly green, and quite opake; but the densest part of the green matter adhered to the fishes themselves, which always swam on the top of the jar. I did not examine this air till the 15th of July, when I found four ounce measures of it, and tolerably pure, but not so much so, I am persuaded, as I should have found it some time before. With two equal quantities of nitrous air, the measures of the test were 1.24.

A quantity of *beef* exposed to the sun in a vessel of water soon became green, and yielded air; but presently the green matter, which had been diffused through the whole mass of water, became yellow, or white; and from that time no more air was produced. The flesh was putrid and offensive. The green vegetable, I doubt not, was quite

quite dead, through the extreme putridity of the flesh, and the foulness of the water, which it had not been able to purify.

To try the difference between the effects of *light* and *darkness* with an animal substance, as I had done before with vegetables, on the 17th of July, I put 8 dwt. 10 grains of roasted beef into a vessel containing about 30 ounces of water, and placed it in the sun, and an equal receiver, with an equal quantity of the same beef in a dark room. On the twentieth I perceived no appearance that struck me, but on the 21st in the evening, I found the flesh in the sun quite green, and two or three ounce measures of air were generated; but the water in the dark room continued quite transparent, and in every respect that I could perceive, unchanged.

On the 26th the green colour of the flesh and of the water in the sun began to disappear, and the vessel had a cloudy appearance. Soon after I examined the air, and found eight ounce measures, very pure, the flesh soft and putrid, but still green on its upper surface. The jar which had been

placed in the dark never had any air, nor was any produced from it afterwards, when it was removed into the sun.

On the 17th of August I exposed in the sun, in a large retort of rain water, 3 dwts. 6 grains of roasted beef, the neck of the retort being plunged in water nine inches deep in a jar that nearly fitted it, and moreover closed with a cork, in which was a very small perforation, so as to give it as little communication with the external air as possible.

On the 9th of September I took from it two thirds of an ounce measure of air, all inflammable. The flesh had never turned green. During the same time I had exposed 8 dwts. 6 grains of the same beef in a jar containing 200 ounces of pump water, which had turned green and yielded dephlogisticated air. In the former case the beef was more in proportion to the quantity of water, and had also a very obstructed communication with the external air, from which alone the seeds of this green vegetable could come.

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This process with a small quantity of *veal* was very remarkable, as this substance continued to be green, and give air, till every thing in it that could be offensive was quite exhausted.

On the 28th of June I put 14 dwts. of boiled veal into a large jar of rain water, and on the third of July both the upper part of the veal, and all the water, was quite green. On the fourth of July I took out half of the veal, and examining the air, I found it to be nine ounce measures, no part fixed air, and so pure that, with two equal quantities of nitrous air, the measures of the test were 0.82. The water was still very green.

Part of this veal, which was then quite soft, I replaced in a jar of fresh water, putting the remainder of it into a small jar. This never gave any air at all. But on the 18th day of July the water in the large jar was all very green, and in two days yielded five or six ounce measures of air. A little time after I examined it, and found twelve ounce measures, so pure that, with two equal quantities

quantities of nitrous air, the measures of the test were 0.57. The flesh had no coherence and still was offensive; but on the 29th of July I took from it four ounces of air equally pure with the former, and on the sixteenth of August half an ounce more, and then the jar had nothing offensive in it.

The process with a roasted *tendon* of a calf's neck went on just as the above, with this difference, which I thought to be a little remarkable, that all the water was of a reddish hue before it became green, though there was no blood, or any thing red, in or about the tendon. The air which it yielded afterwards was very pure.

Perhaps the most satisfactory experiments that can be made with respect to the production of pure air, by means of this green vegetable substance, the pabulum that putrefaction affords it, the effect of light upon it, and again the influence of putrefaction to destroy that air, were some that I made with a *mouse*, which I always found most effectual for any purpose in which putrefaction was required,

required, far more so than pieces of solid meat, of any kind.

On the 21st of June I put a dead mouse into a jar containing 200 ounces of water, inverted in a basin of the same, which I placed in the sun. At the same time I put another mouse into a jar of the same size, filled with the same water, and placed it in the dark. In this vessel the water was never discoloured, and very little air was produced; whereas from the mouse in the sun there presently issued a quantity of white mucous substance, which soon turned to an intense green, and yielded air most copiously. After some time the whole jar was full of this thick green matter, and air rose from every part of it; but it was destroyed as soon as it approached the upper part of the jar, where the dead mouse floated, owing no doubt to the phlogistic matter which issued from it.

In order to verify this, I threw out the mouse, and dividing the turbid green water into two parts, I put one half of it into a retort exposed to the sun, and the other
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into an equal retort which I placed in the dark. The water in the sun presently yielded permanent air, highly dephlogisticated; whereas that in the dark gave not a single bubble, but when I soon afterwards brought it into the sun, it yielded air like the other.

The preceding experiments being made chiefly with the *muscular parts* of animals, I had the curiosity to try what difference would be made with the other parts of the system, and some of the secretions; but I was contented with a few articles under this class, as the extension of the experiments to all the parts of the animal system would have been tedious, and did not seem to promise much advantage.

By means of a quantity of the *brain* of a sheep, and also of the *lungs*, and of the *liver*, I procured a very considerable quantity of very pure air, the process with each of these being exactly like those which have been already described, and therefore not requiring to be repeated. These substances immersed in rain water were presently covered with the green vegetable matter, which
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was also diffused through the whole body of the water, and the produce of air from it was very copious.

The experiments I made with *blood*, *fat*, *gall*, and *gravy* had different results.

Eighteen pennyweights of the crassamentum of sheep's *blood*, was exposed to the sun in a jar of rain water, containing 200 ounces; but it was always red, and never yielded more than an ounce measure of air, the whole of which was phlogisticated.

No air at all was produced from a small piece of *fat mutton*, exposed in the same manner ten days, nor from water which had a small quantity of mutton *gravy* in it.

About half an ounce of sheep's *gall* was exposed, together with the gall-bladder in which it was contained, on the 25th of July, in a vessel containing 200 ounces of water, which in a few days was green, and produced air; but before the 16th of August it was almost all absorbed, and some time after was wholly so. Gall, being a very putrescent substance, might act as the mouse in the experiment recited above; so that perhaps

haps with a less quantity of gall, or by withdrawing it in time, I might have succeeded better.

It is impossible not to observe from these experiments, the admirable provision there is in nature, to prevent, or lessen, the fatal effects of putrefaction, especially in hot countries, where the rays of the sun are the most direct, and the heat the most intense. For whereas animal and vegetable substances, by simply putrefying, would necessarily taint great masses of air, and render it wholly unfit for respiration, the same substances putrefying in water, supply a most abundant pabulum for this wonderful vegetable substance, the seeds of which appear to be in all places dispersed invisibly through the atmosphere, and capable, at all seasons of the year, of taking root, and immediately propagating themselves to the greatest extent. By this means, instead of the air being corrupted, a vast addition of the purest air is continually thrown into it.

By this means also stagnated waters are rendered much less offensive and unwholesome

some than they would otherwise be. That froth which we also see on the surface of such waters, and which is apt to create disgust, generally consists of the purest dephlogisticated air, supplied by aquatic plants which always grow in the greatest abundance, and flourish most, in water that abounds with putrid matter. When the sun shines these plants may also be seen to emit great quantities of pure air.

Even where animal and vegetable substances putrefy in *air*, as they have some moisture in them, various other plants, in the form of *mold*, &c. find a proper nutriment in them, and by converting a considerable part of the phlogistic effluvium into their own nutriment, arrest it in its progress to corrupt the surrounding atmosphere. So wonderfully is every part of the system of nature formed, that good never fails to arise out of all the evils to which, in consequence of general laws, most beneficial to the whole, it is necessarily subject. It is hardly possible for a person of a speculative turn not to perceive, and admire, this most wonderful and excellent provision.

SECTION VI.

Of air produced by substances putrefying in water.

THE experiments recited in this and the following section were entered upon chiefly to discover the *principle of nutrition* in vegetable and animal substances; and they seem to lead us to suppose, that this principle is phlogiston, or the principle of inflammability, in such a state as to be capable of becoming, by putrefaction, a true inflammable air, but not generally such as to burn with explosions, but rather with a blue and lambent flame, mixed with a certain proportion of fixed air.

In the putrefactive process the phlogiston is merely evolved, and not again combined with any thing, except what may be necessary to its assuming the form of inflammable air; but in nutrition it is immediately held in solution by the gastric juice, and in the chyle formed by it. But if any part
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of the aliment pass the stomach, and the first intestines, without having all its phlogiston incorporated with the chyle, that principle remains in the excrement, where it is often set loose in the form of inflammable air, the same form that it would have taken if it had gone through the simple putrefactive process. The phlogiston of the aliment, thus entering into the circulation with the chyle, after answering purposes in the animal œconomy which are yet very imperfectly known to us, is thrown out again by means of the blood in the lungs, and communicated to the air, which is phlogisticated by it.

All alimentary substances not only contain phlogiston, but I believe are capable of yielding a proper inflammable air by putrefaction. But in the following experiments on such vegetables as are generally used for food, *roots* seem to yield it in a greater abundance than other parts of plants; but there are some remarkable differences among them in this respect. For though it was seen in the last section, that potatoes are exceedingly fa-

vourable to the growth of that green vegetable substance, which yields pure air so copiously, owing probably to the phlogiston they contain, *onions*, perhaps equally nutritive with potatoes, are exceedingly unfriendly to that plant; but then they yield inflammable air in an astonishing quantity, when they are left to putrefy in water. This I rather suspect is a proof, that onions contain more phlogiston, and are the more nutritive substance of the two.

On the 28th of June I exposed to the sun 18 dwts. of onions, in a jar of 100 ounces of river water, inverted in a basin of the same. They presently began to yield air, but without ever becoming green; and on the 15th of July the quantity was fifteen ounce measures, a small part of which was fixed air, and the rest strongly inflammable. The water was white and turbid, and the air had a strong smell of onions.

About the same time I observed that it made no difference, with respect to the quality of this air, whether the onions were placed in the light or in the dark, the principle

ciple of vegetation not being concerned in this case. And though I observed the following differences in the quantities of air produced in the sun and in the shade, they were not uniform, and therefore must have depended upon some unknown accidental circumstances.

On the 17th of July I put two onions, each weighing an ounce and a quarter, in the sun, and two others of the same size, in a similar jar in the dark. On the 23d I examined them, and had 24 ounce measures of air in the shade, and only 12 from those in the sun; but the latter was more strongly inflammable than the former, which burned with more of a lambent flame, though both exploded in some measure, so as to be something more inflammable than air from marshes.

Having kept a quantity of this air, from the time above-mentioned to the 20th of July 1780, I found it then strongly inflammable, little inferior to the inflammable air from metals. Perhaps the fixed air, which had been mixed with it before, was now com-

pletely expelled from it. It appears, however, that this kind of inflammable air has an inflammability of as permanent a nature as any whatever. The air from marshes also, which, with Sig. Volta, I doubt not comes from putrefying vegetable substances, I have also found to be equally permanent.

On the 1st of August I took two halves of the same onion, (which was an old one, and beginning to sprout) each half weighing 17 dwts. 12 grains, and I placed one of them in the sun, and the other in the shade, both in similar receivers. On the 24th of the same month, that in the sun had given an ounce measure and three-quarters of air, of which one-fifth was fixed air, and the rest inflammable. From that in the dark I took $2\frac{1}{4}$ ounce measures of air, one third of which was fixed, and the rest inflammable. From these experiments I was ready to conclude, that onions (and therefore, probably, other vegetable substances) would always give more air in the dark than in the light; but the following experiments shewed that this is by no means the case always.

The

The 30th of July I placed in the sun, in a vessel containing fifty ounces of water, a part of a fresh gathered onion, weighing 9 dwts. and also another part of the same onion, and of the same weight, in a vessel of the same size in the dark. On the 24th of August that in the sun had yielded three ounce measures of air, all inflammable, and that in the dark had produced as nearly as possible the same quantity, and as inflammable, when the fixed air that was mixed with it was washed out of it. The fixed air which had been extricated in the sun had been dissipated by means of the free access of fresh air.

Upon a former occasion I got only fixed air from onions confined by quicksilver; but then they wanted moisture, or were not kept till they were properly putrid. For I have since got inflammable air, as well as fixed air, from onions kept in quicksilver, from the 2d of September, 1779, to the 31st of March, 1780. The onions weighed 12 dwt. 20 gr. and the air was half an ounce measure, three fourths of which was fixed air, and the rest inflammable. It

appears from this, as well as many other observations which I shall have occasion to mention hereafter, that neither fixed air, inflammable air, or nitrous air, can be produced without a considerable quantity of water, part of which we may therefore probably infer enters into the composition of these kinds of air; though when they *are* formed, I know no method of discovering, and reproducing that water.

Both *carots* and *parsnips* yield great quantities of inflammable air, and equally in the sun or in the shade. I was at one time much amused with observing the inflammable air issuing from one of the carots in the sun. It came sometimes in a constant stream, or in large successive bubbles, from one particular place, neither at the centre, nor near the outside of the carot, but in the place where the air holes are the largest.

To ascertain the quantity of air produced from a given weight of these two roots, I placed as much of a parsnip as, by expelling water from a cylindrical vessel, I found
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to occupy the space of $2\frac{1}{4}$ ounce measures of water, in the sun; and the next day I took from it four ounce measures of air, all fixed air, the residuum extinguishing a candle. This was on the 29th of July, and on the 31st of the same month I took from it four ounce measures more, of which two thirds of an ounce measure was inflammable. On the 2d of August I again took from it four ounce measures, one fourth of which was inflammable, exploding with a blue flame. Lastly, on the 24th of August, perceiving that no more air would be produced, I took from it one third of an ounce measure; one third of which was fixed air, and the rest not inflammable, but phlogisticated.

From carrots occupying the space of an ounce measure and half of water, exposed to the sun in rain water, from the 26th to the 31st of July, I took ten ounce measures of air, of which an ounce measure and half was strongly inflammable exploding with a red flame; and on the 4th of August I took from them near four ounce mea-

fures of air, of which more than one half was inflammable. The water, which had a large surface, had probably absorbed much of the fixed air. This, however, was all the air that these carots would yield.

An equal weight of carots, exposed the same time in the dark, yielded nearly the same quantity of air, but only a small proportion of it was inflammable. This, however, I do not attribute to the darkness, but but to some other unknown circumstance.

A sliced *turnip* fresh gathered, weighing near three ounces, exposed in the sun in rain water, yielded twelve ounce measures of air, one third of which was fixed air, and the rest strongly inflammable.

On the 30th of July two ounces of turnip, fresh gathered, were placed in the dark, in a vessel containing seventy ounce measures of water; and on the 24th of August I took from it an ounce measure and a quarter of air, of which one ounce measure was phlogisticated, not inflammable. The water was exceedingly offensive. This phlogisticated air had been, I doubt not, inflammable

in its origin, and in much greater quantity. When a turnip was sliced very thin, and the quantity of water large, I have observed before, that dephlogisticated air was produced.

Fruits, I found by no means favourable to the production of pure air. Like the preceding roots, they putrefied, and yielded inflammable air, mixed with fixed air. From *peaches*, both in the sun and in the shade; I got air, three fourths of which was fixed air, and the rest inflammable; but on this occasion the quantity of air produced in the sun was twice as much as that produced in the shade; though the quantity of water in which they were exposed was the same, and the peaches themselves were, as far as I could perceive, of the same size, and in the same state.

I placed two *Morella* cherries, one in the sun, and the other in the shade, in equal vessels of water. From that in the sun I got one third of an ounce measure of air, and from that in the shade one fifth of an ounce measure, both inflammable. I had the same result with apricots.

Having

Having found the capacity of these nutritive substances to yield inflammable air, I next tried whether they would part with any of it in *boiling*. But I found that none of them did, but only in *putrefying* afterwards; so that this mode of preparation (and the same I doubt not would be found to be the case with roasting, &c.) does not deprive any of these aliments of any part of their nutritive power.

From 19 dwts. 18 grains of *onions* I expelled, by boiling in river water, half an ounce measure of air, of which one third was not absorbed by water, and extinguished a candle.

From one ounce 15 dwts. of *lettuce* I got three quarters of an ounce measure of air, of which half an ounce measure was phlogisticated air.

From 1 oz. 16 dwts. 12 grains of *carots* I got three quarters of an ounce measure of air, of which about one ounce measure was phlogisticated air.

These differences are inconsiderable, and some of the air, no doubt, came from the
water

water in which these substances were boiled.

Afterwards the potatoes and carrots, putrefying in water, yielded each more than two ounce measures of air, one half of which was fixed air, and the rest inflammable. The onions yielded only about half an ounce measure of air, but it was of the same kind, and the lettuce gave only a tenth of an ounce measure, in which nothing could be perceived to be inflammable. But I did not begin to collect this air till a day or two after the process of boiling, when I perceived some of the substances to be in a state of yielding air.

SECTION VII.

Of air produced by various substances putrefying in quicksilver.

AT the same time that I was endeavouring to find what quantity, and what kind of air, various substances would yield by putrefaction in water, I was willing to ascertain the production of air from them, and from other substances, putrefying in quick-silver. I find, however, that all that I thought worth registering were the experiments made with some animal substances. A few similar experiments on vegetables I have occasionally noticed elsewhere.

By means of these experiments, and those in the preceding section, it may be possible to determine the nutritive powers of different vegetable and animal substances, and also other problems in philosophy; though too much must not be expected from them.

It might have been imagined, that by this means we should be able to ascertain
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the quantity of air that any mass of putrescent matter would thoroughly phlogistificate. For any given quantity of inflammable air will completely phlogistificate twice its bulk of common air. But it will be found that a putrefying mouse will phlogistificate much more than that proportion of air. There must, therefore, be much more phlogiston issuing from a mouse than forms the inflammable air which comes from it. Perhaps therefore that phlogiston which contributes to animal nutrition, may also be more than that which enters into the composition of the inflammable air that comes from the putrefying substance. This is a subject that requires and deserves much farther investigation. I only recite the following as *leading experiments*, to the solution of greater problems. They are, indeed, upon too small a scale to be of much use even for this purpose; except to shew that the same kind of substance, which in a large quantity yields inflammable air, in a small quantity may yield phlogisticated air.

A small

A small *fish*, weighing 1 dwt. 20 grains, being confined in quick-silver from the 21st of May to the 24th of August, gave something more than half an ounce measure of air, two thirds of which was fixed air, and the remainder extinguished a candle, but was not sensibly inflammable.

From 2 dwts. of well boiled *beef* I got a very small quantity of air, the bulk of which was fixed air, and the rest not inflammable. At another time, from 1 dwt. 19 grains of *raw beef*, I got 0.22 of an ounce measure of air, nine tenths of which was fixed air, and the rest extinguished a candle.

From 2 dwts. 5 grains of *raw lamb*, I got 0.17 of an ounce measure of air, the bulk of which was fixed air, and the rest not sensibly inflammable: but from 2 dwts. 2 grains of well *roasted lamb*, I got three quarters of an ounce measure of air, half of which was fixed air, and the rest highly inflammable; and some time after I took from the same substance half an ounce measure of air more, of which three fourths was fixed air, and the rest inflammable.

From

From 13 dwts. 4 grains of the *tendon* of a roasted neck of veal, I got an ounce measure and half of air, of which half was fixed air, and the rest phlogisticated. Afterwards I took from it one ounce measure and three quarters of pure fixed air, with the smallest residuum possible. In the former experiment also, as well as on a former occasion (mentioned vol. 3. p. 343.) I found that the inflammable air was extricated first, and a long time before all the fixed air was exhausted.

Having had occasion to make many experiments with putrefying *mice*, and having more in prospect, I was particularly desirous to ascertain the quantity and quality of the air produced by a mouse of the middle size putrefying in quick-silver, and I found as follows. A mouse weighing 6 dwts. 3 gr. confined by quicksilver, which had putrefied from the 8th of April, had yielded on the 24th of July one ounce measure and three-quarters of air, of which one fourth was weakly inflammable, and the rest fixed air. This I found, by other experiments, was
nearly

nearly as much as a mouse would yield in these circumstances.

Having left another mouse to putrefy in quick-silver, I took the air produced from it at different times, in order to satisfy myself more fully with respect to the proportion that the fixed and inflammable air bore to each other, from the beginning to the end of the process. The mouse weighed five dwts. 10 grains, and it was put into an inverted vessel of quick-silver on the 13th of June. On the 26th of that month, I took from it near an ounce measure of air, three-fourths of which was fixed air, and the rest inflammable, burning with a very blue flame. On the 16th of August I took from it an ounce measure and a quarter of air, of which four-fifths was fixed air, and the rest, if it was inflammable at all, was so in the slightest degree imaginable; and lastly, on the third of April following, I took from it a small quantity of air, perhaps one-tenth of an ounce measure, the whole of which was, as far as I could judge, all fixed air.

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When a mouse is left to putrefy in this manner, there comes from it a great quantity of dissolved blood, or some other thin reddish liquor. This I carefully separated from what was *solid* in the mouse, and found that this continued to give air, when the liquor gave little or none; so that perhaps it may be something *solid* in all bodies that contributes to the formation of permanent air. By long standing, however, I did get a little air from this red liquor, and it was almost all fixed air. It was perhaps combined with it, at its separation from the mouse.

The experiments on some of the different *parts* and *secretions* of animal bodies were made on the same small scale with most of the preceding, and therefore they can only have the same imperfect use.

From 7 dwts. of the medullary part of a sheep's *brain* raw, I got $4\frac{1}{2}$ ounce measures of air; of which one fifth part of an ounce measure was inflammable, and the rest fixed air. I also found by similar experiments, that the cortical part of the same brain gave somewhat less air than the medullary part;

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but the proportion of the inflammable to the fixed air was the same. No certain inference, however, can be drawn from experiments on so small a scale as these.

Two dwts. of *mutton gravy* yielded 0.02 of an ounce measure of air, the greatest part of which was fixed air, and the remainder seemingly inflammable.

Two dwts. of the *crassamentum* of sheep's blood gave only a small bubble of air, too small to be examined. The *serum* also yielded some air, the bulk of which was fixed air, and the rest phlogisticated.

An ounce measure of *milk* yielded near half an ounce measure of air, almost pure fixed air, a small remainder being phlogisticated.

An ounce measure and an half of the *bile* of a sheep yielded half an ounce measure of air, almost all fixed air, the small residuum being phlogisticated.

I should not have made these experiments on so very small a scale, but that I expected a greater quantity of air from all the substances, and because less quick-silver was wanted for the purpose; so that I could have
more

more processes going on at the same time. Had the same substances putrefied in *water*, they would have yielded many times more air.

S E C T I O N VIII.

Of the production of inflammable air from iron filings and brimstone made into a paste with water.

AT the time of my last publication, having put a pot of iron filings and brimstone into a jar of nitrous air (the first effect of which was to reduce it to one fourth of its bulk, and leave it in the state of phlogisticated air) and having some time after this found the air much increased in quantity, and strongly inflammable, I had some doubt whether the inflammable matter came from some farther change in the nitrous air, or from an exhalation of proper inflammable air from the iron and brimstone. My doubt

arose from my never having found that this paste of iron filings and brimstone, whether kept in water, or in vacuo, had yielded air at any time, except in a considerable degree of heat. In consequence, however, of repeated experiments, I am now satisfied, that the inflammable air came from this mixture. For though some pots of it have not yielded inflammable air, they have all, with *long keeping*, even in the temperature of the atmosphere, yielded either phlogisticated or inflammable air; the latter generally when the composition was fresh made, and the former when it was old.

These experiments have also led me to the observation, that, in this and many other cases of the diminution of common air by phlogistic processes, a true inflammable air is first produced, and in its *nascent state*, as it may be called, is immediately decomposed, previous to the phlogistication of the common air. The very same substances which, in water or quick-silver, yield inflammable air, only phlogistificate common air: so that I am almost ready to conclude universally, that

that air is never phlogisticated, but by materials which, in certain circumstances, would yield inflammable air; though when inflammable air is previously produced, and then mixed with common air, it will not be decomposed in the temperature of the atmosphere, except in a very small degree. These two kinds of air will, therefore, continue mixed without much affecting each other, except in a red heat, by which the inflammable air is fired. It is then well known to cease to be a separate inflammable air, the phlogiston being separated from it, and entering into the composition of the phlogisticated air, into which the common air is now changed; when both the whole of the inflammable air disappears, and likewise about one fourth of the common air along with it.

The experiments which led to these conclusions, and which I shall now proceed to recite, may serve as a caution to myself and others, not to be too hasty in drawing general conclusions; since what may appear to be the *same materials*, and the *same prepara-*

tion of them, may have different results, in consequence of there having been some circumstance, respecting either the materials or the process, that was unnoticed, but which was the secret cause of the unexpected results.

That nitrous air might be changed into inflammable air, was not extremely improbable *a priori*; since I had found that it contained as much phlogiston as inflammable air, bulk for bulk; and since it is, by several processes, convertible into what has the appearance of a species of inflammable air. Besides, in this very case, the same composition of iron filings and brimstone, which I now find generally yields inflammable air in the temperature of the atmosphere, does not do so at all times.

Thinking that if the iron filings and brimstone had really yielded the inflammable air which I found in the vessel of nitrous air, it would do the same in common air, I confined a large pot of this mixture in a very small quantity of common air in the beginning of February 1779. But though
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on the 19th of May following it was increased in bulk, it was all mere phlogisticated air, and had nothing inflammable in it. Even the air that was entangled within the cavities of this pot of iron filings and brimstone, and which I caught by breaking it under water, was not inflammable. It is possible, however, as I observed before, that this phlogisticated air might have been inflammable air in its origin, or *nascent state*, and have become phlogisticated air afterwards. At another time I put a pot of this mixture under water, as I had done formerly, and now also observed, that though it fermented very well, and turned black, yet it did not yield a particle of air in about a fortnight: and in experiments of this kind few persons, I believe, would look for any farther change beyond that time.

Soon after, however, I found that a pot of this mixture, fresh made, and kept under water three weeks, had yielded about its bulk of air; and this was strongly inflammable. But at the same time another mixture of this kind, kept in the same

circumstances, yielded only phlogisticated air; and yet I did not knowingly make any difference in the composition, always mixing equal bulks of the two ingredients.

As the phlogiston which constituted the inflammable air in the experiments that occasioned these must probably have come from the iron, and not from the sulphur; especially since iron alone is capable of making a very remarkable change in nitrous air, I confined a quantity of this air, in a vessel full of iron nails, from the beginning of February to the 18th of May; but after this long interval it was only phlogisticated air, and not in the least inflammable.

Having found, however, that this mixture of iron filings and brimstone was capable of producing inflammable air in water, I made a trial of it in quick-silver, and found it to have the same effect. For confining a quantity of this mixture in quick-silver from the 13th to the 30th of June, in the temperature of the atmosphere, it had yielded, in this time, its own bulk of air, strongly inflammable.

I found

I found afterwards, in a proper number of trials, that in a sufficient space of time, this mixture increased all the kinds of air into which I introduced it, by the addition of a quantity of inflammable air, more or less, according to circumstances known or unknown. But when the experiment was made in common air, it first diminished it about one fourth, as I have often noted; and some time after that I perceived an addition made to the bulk of the air, and examining it, found it at first to be slightly inflammable, but afterwards more strongly so. This experiment shews that, in the first instance, the inflammable air yielded by iron filings and brimstone must have been decomposed in phlogisticating the common air, before it could appear in its proper form.

It appeared upon one occasion, recited above, that one pot of this mixture, *fresh made*, produced inflammable air, at the same time that a pot of an *old* mixture of this kind yielded only phlogisticated air. But at what time these mixtures will cease to give inflammable air, and begin to yield phlogisticated

ticated air, I cannot determine. For I find that on the 23^d of June a pot of iron filings and brimstone, which must have been mixed about a year before, confined in a small quantity of common air, had made an addition to it of three ounce measures on the 26th of July; and this air was inflammable. At the same time I found that another quantity, which had been mixed the 1st of July, had yielded inflammable air, in about the same proportion, according to the time. Also some old iron filings and brimstone, which had been taken out of the pot, and mixed with water the third of July, had yielded about one tenth of its bulk of air on the 2^d of August, strongly inflammable.

That future experimenters may form some idea of the quantity of inflammable air that they may generally expect from such mixtures as I usually made of iron filings and brimstone, using equal bulks of of each, and therefore be less apt to deceive themselves in the results, I shall recite the issue of some that I made with this and other mixtures,

mixtures, and which I was obliged to put an end to when I removed my habitation on the 21st of July, 1780.

A gallipot, containing an ounce measure and half of this mixture, having been confined, in a small quantity of common air in the beginning of July 1779, had at the time above-mentioned produced fourteen ounce measures of air, strongly inflammable, but the production was much more rapid at the first than afterwards. The mixture was very hard.

Another gallipot of the same size, put into a vessel of water, without any air, on the 23d of June, 1779, had three ounce measures of inflammable air taken from it on the 26th of July following, and at this time there were eleven ounce measures, strongly inflammable. The mixture was very soft.

Another equal quantity had yielded strong inflammable air from the 24th of June to the 15th of July 1779, and had from that time yielded about three ounce measures of air, but slightly inflammable. The mixture was very soft.

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There is the same uncertainty attending experiments made with *liver of sulphur*, which also exhales phlogiston, and produces the same effect both on common air and nitrous air, as iron filings and brimstone. On the 19th of May 1779, I found a quantity of nitrous air, in which some liver of sulphur had been confined from the 12th of December preceding, and which was considerably increased in bulk, to be strongly inflammable; and yet another quantity of this substance, and fresh made, was confined in quick-silver several months without producing any air at all.

Having been led by some of the preceding experiments to expect, and even to believe, that common air is usually phlogisticated by actually decomposing a small quantity of inflammable air, admitted to it in its nascent state (notwithstanding large quantities of inflammable air ready formed have no sensible effect upon it) I wished to ascertain so extraordinary a fact, by some experiments of a more decisive nature, and with that view I made the following.

First

First I took a pot of iron filings and brimstone, which I had found to have been in a state of yielding inflammable air in water about three months, and which I therefore presumed would continue for some time in the same state. This pot being introduced to a quantity of common air made no addition to it, but diminished it, and phlogistigated it as usual.

I then took a quantity of this mixture, which had yielded inflammable air many months, in a vessel of water. On the 22d of September I introduced some common air into the vessel in which it was contained, and on the 26th of October I observed that, though this mixture, now covered with water, had thrown up bubbles of air, which mixed with the common air on the surface of the water, that air was sensibly diminished, though not more than one tenth in all, and being examined was found to be phlogistigated, and to have nothing inflammable in it. At the same time a quantity of dephlogistigated air, exposed in a similar manner, was diminished from $11\frac{1}{2}$ to one third; and
from

from having been very pure, the measures of the test, with two equal quantities of nitrous air, were now 1.24. A candle burned in it better than in common air, but there was nothing inflammable in it.

But the most decisive experiment that I made of this kind was the following. A quantity of iron filings and brimstone were mixed, and put into a phial filled up with water, on the 24th of June 1779, and on the 25th of July following it had yielded a quantity of inflammable air, which was then all taken out; and on the 22d of September more inflammable air was produced, about two ounce measures in all. Having by this means satisfied myself that this mixture was in a state of yielding inflammable air, I introduced to it a quantity of common air (the phial having been always kept inverted in a basin of water) and on the 26th of October I found the common air very considerably diminished. This air then being thrown out, the mixture was kept in the phial, now filled with water. In these circumstances, it continued to yield air, and
when

when an ounce measure and an half was produced, which was on the 24th of March 1780, I examined it, and found it to be strongly inflammable. There could therefore be no doubt but that the common air in this experiment had been diminished and phlogisticated by an addition of inflammable air in its nascent state, or rather after it was completely, though but *newly formed*.

I do not by any means infer that, because common air was diminished in this case, in consequence of an accession of inflammable air in its nascent state, that it is never diminished in any other manner; but perhaps it will be found that all the substances which we know to phlogisticate common air are likewise capable of yielding inflammable air, if not in the temperature of the atmosphere, at least by means of *heat*, or in some other process. This I found to be the case with metals, and it has been seen to be so, in one instance, with liver of sulphur, and it is remarkably the case with all putrefactive substances. Vaults containing human excrements are often found to abound with
inflammable

inflammable air, and they, like other putrefactive substances, diminish common air.

The preceding experiments on the phlogistification of common air by means of inflammable air, led me to try whether a degree of heat short of ignition would not make a quantity of inflammable air, ready formed, part with its phlogiston to common air. For this purpose I mixed two thirds of common air with one third of inflammable air, and I kept them as hot as I could do without melting the glass vessel in which they were contained several hours; but afterwards they occupied the same dimensions as before. This air was confined in a glass jar, the upper part of which was surrounded with hot coals, by means of the instrument described in my last vol. fig. 4.

I then tried what *length of time* would effect in this case; but though I always found a very sensible, and sometimes a considerable diminution, the phlogistification was never completed, and the progress of it always stopped, without ever proceeding farther in any length of time afterwards. The following

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ing facts in proof of this it may be just worth while to recite:

A mixture of one third inflammable air and two thirds common air, which had been confined by water from the 30th of June 1779, was diminished the 18th of March 1780 one twelfth, and burned with a lam-bent blue flame.

A mixture of one third inflammable air and two thirds common air, from some time in the month of June 1779, was diminished one thirteenth on the 20th of July 1780; but it had been diminished nearly as much on the 5th of October preceding. It burned with a considerable explosion.

Another quantity, one third inflammable air from marshes, and two thirds common air, from the same time, was diminished one tenth; but on the 18th of March preceding it was diminished one twelfth, and then burned with a blue flame. At this time also, viz. the 20th of July 1780, it burned in the very same manner.

Exactly similar to these were the experiments that I made with various mixtures of

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dephlogisticated air and inflammable air.

Air one half inflammable and one half dephlogisticated, mixed in June 1779, was on the 20th of July 1780 diminished one fifth; but it had been diminished two thirds of it the 4th of October preceding. It burned with a considerable explosion.

Another mixture of the same kind, and made at the same time, was, on the 3d of July 1779, diminished two elevenths, and on the 1st of January 1780, it was diminished two fifteenths more. On the 20th of July 1780, it was in the very same state, and burned with a very considerable explosion.

I had, however, better success when, in imitation of an experiment recited above, I admitted the inflammable air to the common air at different times, and in very small quantities. This I did every day, and at the same time kept the vessel containing it very near the fire. I took no exact notice of the quantity of inflammable air that I mixed with it in all, but the bulk of the common air was not diminished by this addition. Nothing inflammable could be perceived in
it,

it, and yet it was so far phlogisticated, that, with an equal quantity of nitrous air, the measures of the test were 1.36; when, with the same nitrous air, and the common air, the measures were 1.26.

I do not know, therefore, but that, with much patience, admitting a small bubble of inflammable air every day, to a large quantity of common air, it might not be both diminished in bulk, in the usual proportion of one fourth of the whole, and completely phlogisticated, even in the temperature of the atmosphere. Nothing, however, but experiment, can decide in this case.

SECTION IX.

Of the air that has been supposed to come through the pores of the skin, and of the effects of the PERSPIRATION of the body.

I HAVE sometimes found it necessary, though it is by no means agreeable to me, to correct the mistakes of others on the subject of which I am treating; and I must appropriate this section to that business.

It cannot be thought extraordinary, that when it has been imagined that air is extracted from the most compact bodies, as gold, by means of the air pump, it should be thought to issue from the human skin. It was also very natural to imagine, that since *respiration* injures and phlogisticates air, the *perspiration* of the body, sensible and insensible, should do the same; and they who suppose that phlogiston converts common air into fixed air, must of course imagine, that the air contiguous to the skin is continually undergoing this change. Dr. Ingenhoufz asserts

asserts the former, and Mr. Cruikshank, after Sig. Moscati, the latter. On both these subjects I shall make some animadversions, and likewise a few experiments that I think will be deemed conclusive, on the subject of perspiration, and sufficient to confirm what I have advanced with respect to it in my last volume.

Dr. Ingenhoufz not only supposes that air is continually issuing from the human skin; but he took pains to collect it, in a considerable variety of circumstances, of which he has given a particular account p. 129. This I took the liberty to tell him I had no doubt was a deception; the air that he found not having come from the *skin*, but from the water in which it was plunged: and both the quality of the air that he found, and the circumstances in which he procured it, left me no doubt upon the subject. It was just that mixture of fixed air and partially phlogisticated air, that pump water, which he recommends for the purpose, generally abounds with. The bubbles of air rising and swelling at the same part of the skin

is by no means any proof that the air came from the skin: for that is always the case with air issuing from water, the air bubbles never rising within the water itself, but always from some other body immersed in it. All the phenomena he has described may be seen with a piece of metal, or glass, plunged in water containing air, in an exhausted receiver; in which case it is easily shown, that the air does not come from the pores of the metal, or of the glass, but from the water itself; for if the water contain no air, and the surfaces of the metal and of the glass be carefully wiped, that appearance cannot be produced.

He says that water exhausted of its air is not proper for this experiment, because it readily absorbs all the air as fast as it issues from the skin. But if the experiment be made in water at all, this must be the only unexceptionable manner of making it; and water by no means absorbs any kind of air so fast as he describes this to issue from the skin, and especially such a kind of air as he describes, a great proportion of which is air

partially

partially phlogisticated. It requires a long time before water, in a quiescent state, will take up any sensible quantity of such air as this. Besides, there is nothing that we know of the human frame, that would lead any person to suspect that air ever issues from the skin. Where are the *air vessels* for that purpose? and what is their origin, or connection with other parts of the system? The present state of anatomy indicates nothing on this subject.

To satisfy my friend, not myself, I told him I would make an experiment, which I did not doubt would convince him of his mistake in this respect: I did it in the following manner. I boiled a quantity of rain water, in order to expell from it all the air it might contain, and then sat with my naked arm plunged in a vessel filled with it, after carefully wiping off, under water, all the bubbles of air that adhered to it. But though I continued to sit in this manner a full half hour, not a single bubble of air made its appearance afterwards. I might have examined whether this water had contained any air,

besides what it might have been supposed to have imbibed from the atmosphere in this interval; but I neglected to do it, and am very confident it was quite unnecessary.

After this I need not say any thing to my friend's ingenious observations on the air which he took the pains to collect from the skins of old and young persons, and his laudable endeavours to remove a popular prejudice concerning the unwholesomeness of the former, and the wholesomeness of the latter kind of air.

Mr. Cruikshank's experiments, if they could be depended upon, would both prove that fixed air is composed of common air and phlogiston, and that the perspiration of animal bodies, in a healthy state, has the same effect upon air that breathing it has, viz. phlogisticating it, and making it noxious, which is contrary to the experiments of which I gave an account in my last publication; by which it appears that the air under my arm-pits, and near other parts of my body, was never less pure than the external air. The Abbé Fontana also told me, that he had always
found

found the same result in experiments made upon himself. But Mr. Cruikshank says, (in the second edition of his *Letter to Mr. Clare*, printed in Mr. Clare's Treatise on Abscesses) that, after he had confined his leg in a glass vessel, so as to prevent all communication with the external air, lime water poured into it immediately afterwards, came out a little turbid. But this he would probably have found to be the case with a small quantity of lime water, poured into and out of any vessel of the same size, on account of the great surface of the fluid that must, in those circumstances, have been exposed to the common atmosphere; in consequence of which it is always known to attract fixed air.

However, partly to examine this matter more thoroughly, and with a variation that I had thought of, I repeated the experiments on my own perspiration in various ways, and they all confirmed what I advanced before, viz. that the perspirable matter has no such effect upon the air, but leaves it as wholesome, that is, as fit for respiration, as ever, judging by the test of nitrous air, which,
however,

however, Mr. Cruikshank does not say that he ever applied in this case.

Pursuing his steps, I fastened a moist ox's bladder, containing about a quart of air, close about my ankle, so that my foot, clean washed and warm, as his was, was exposed to it; and I sat near the fire, so as to keep my foot properly warm a full hour. After this I carefully withdrew my foot from the bladder, without changing the air; and applying the test of nitrous air, the air in the bladder appeared to be of the same degree of purity with the external air; the measures of the test, applied in the same manner to both, being 1.26. I also admitted part of this air to lime water, and observed that it did not make it in the smallest degree turbid.

Willing to give more time to this experiment, that the opportunity of this perspiration phlogisticating the air might be the greater, I once more fastened the bladder about my foot, just before I went to bed, and slept with it all night, keeping myself sufficiently warm, from eleven to half past six in the morning, when the bladder was quite

quite dry. However, carefully moistening it, and especially where it was fastned to my ankle, I withdrew my foot, without changing the air, and immediately examined it. The quantity contained in the bladder was 40 ounce measures. It did not affect lime water, and with respect to purity was of the same standard with common air; the measures of the test with the nitrous air I happened to make use of, being in both cases 1.3.

I cannot therefore but see reason to conclude, as I did before, that it is only *respiration*, and not the *perspiration* of the body that injures common air.

SECTION X.

Observations on RESPIRATION, with a view to ascertain the origin of the fixed air discovered by it.

IT is a prevailing opinion, and, notwithstanding the pains I have taken to explain myself from time to time, is by many persons still ascribed to me, that common air, by the addition of phlogiston, becomes fixed air. Mr. Cruickshank, as I have observed, thinks that some experiments of his are decisive in favour of it, and my friend Mr. Kirwan is, I find, inclined to it. As I conceive this hypothesis to have no foundation in fact, it may not, I think, be amiss to animadvert a little upon it.

All the experiments that I have yet heard of, that have been imagined to favour this opinion, only shew that there is an appearance of fixed air when common air is phlogisticated. But this may be the case if any considerable quantity of fixed air be contained

tained in the common atmosphere, either properly incorporated with it, and making part of its constitution, or diffused through it. For the addition of phlogiston, or rather its union with common air, whereby it becomes phlogisticated air (which is quite another substance) may precipitate the fixed air, in consequence of its having a stronger affinity with the basis, whatever that be, of common air. And that fixed air is, in some way or other, contained in common air, is evident from its being imbibed by lime water, whenever it is exposed to the common atmosphere. But besides the fixed air which is thus capable of being attracted by lime water, common air, probably at least, contains a quantity that is held in a much firmer union with it. For when lime water has absorbed all the fixed air that it can from any portion of common air, it is as fit for respiration as ever; and when it is phlogisticated, at least by respiration or putrefaction, a much greater quantity of fixed air is (at least seemingly) precipitated from it.

It is, I imagine, this appearance of fixed air that has led so many persons to suppose that it is formed by the union of phlogiston with common air. But if it be the addition of phlogiston that makes one part of any quantity of common air become fixed air, why does not the addition of more phlogiston convert the whole into fixed air, which is never the case? For in simple phlogistication the diminution never proceeds farther than about one fourth of any given quantity of common air, and the remainder is a thing as remote from fixed air as any kind of air can be; and it is in vain to attempt, by the addition of more phlogiston, to convert it into fixed air.

Besides, considering the great diminution of common air by phlogistic processes, there is no greater appearance of fixed air produced by respiration, than has been supposed to be contained in common air, and to be precipitated from it, even admitting, as I do, that the whole of the diminution is not owing to the precipitation of fixed air. Breathing into lime water seems to have been the principal circumstance

cumstance that has led to the mistake which I am now animadverting upon; but few persons are aware how small a proportion of fixed air is necessary to make a very turbid appearance in a great quantity of lime water.

From these reflections on the subject I was led to make the following experiments; which though they discover new difficulties in it, may serve to give some kind of satisfaction with respect to it, and prepare the way for farther investigation.

It must be allowed to be a curious subject of inquiry, to ascertain the quantity of fixed air naturally contained in a given quantity of common air, or to trace the source of the fixed air which appears in some processes for phlogisticating common air. Now in some of these processes it seems to be more considerable than in others, and in some I find none at all. This remarkable difference, I own, I am not at present able to account for. Let the following facts speak for themselves.

The diminution of air by breathing seems to be less than by putrefaction, or several
other

other processes; and though air is not completely phlogisticated by this means (the animals dying before it quite arrives at that term) yet the diminution seems to be less even in proportion to the degree of phlogistication. The diminution is evidently much greater by means of putrefaction, notwithstanding the emission of permanent air from the putrefying substance, which *a priori* there is no reason to suspect from a living body. To make the following experiments in the fairest manner, I made use of quick-silver, rather than of water, to confine the air.

A mouse being suffered to live as long as it could in a given quantity of air, confined by quick-silver, I let it remain two or three days afterwards; in which time there was no sensible diminution of the air. I then withdrew the mouse; and admitting lime water to the air, it was diminished one twenty-eighth part of its bulk. But the precipitation of the lime was not very considerable. Agitation in water would have produced a farther diminution, as in the following experiment.

A mouse

A mouse having breathed as long as it could in a quantity of air confined by quick-silver, I admitted lime water to it as soon as it was dead, when there was an immediate and copious precipitation of lime. After it had stood two days, one nineteenth of the whole quantity was absorbed; and by agitation in water it was reduced in all one tenth. This air being examined, with an equal quantity of nitrous air, the measures of the test were 1.76. so that it was something short of being completely phlogisticated.

Another mouse dying in an equal jar of air, in the same manner, I kept it upon the quick-silver four days, during which time there was no absorption of any thing; but upon water being admitted to it, one eighth of the whole quantity disappeared; and examining the remainder by nitrous air, the measures of the test were 1.8; which, considering how much of the nitrous air is absorbed by passing through water, may be deemed a pretty near approach to complete phlogistication.

At another time a full grown, but young mouse, lived seven hours, in ten ounce measures of common air, confined by quick-silver. Lime water being then admitted to the air, it became turbid. But when one fifteenth of the whole was absorbed, the remainder seemed to have but little fixed air in it, though the agitation in water reduced it between one fifth and one sixth of the whole. This was the greatest diminution that I ever found in this way.

In these processes it is not easy to determine how much of the diminution is owing to the precipitation of fixed air; but so far is clear from these experiments, that let the matter to be absorbed be what it will, the absorption cannot take place so long as the air is confined by quick-silver, there being nothing, as we may suppose, with which the matter to be absorbed can unite in those circumstances; though it is ready to separate from the rest of the mass of air upon the admission of water, with which it can unite. In the case of respiration therefore, that
which

which is separated from the common air seems to be either all fixed air, or some substance similar to it.

In the next place, I endeavoured to ascertain the quantity of fixed air produced by my own respiration in a given time; and the quantity of air that I could phlogisticate in that time. For this purpose, I put a quantity of lime water into a glass tube, three feet long and an inch wide, filling it so high as that no part of it might be thrown over, when I breathed through it, by means of a small glass tube reaching to the bottom of the large one. In this manner I breathed two minutes. Then carefully pouring out all the turbid water, and filling a phial with it, I poured into it a quantity of oil of vitriol; enough to dislodge all the fixed air from the precipitated lime. However, lest this should not be sufficient, I afterwards expelled all the air that I could from it by means of heat. Then, rejecting all the permanent air that came over along with the fixed air, and allowing, as well as I could, for all that might have escaped, without being seized

by the lime in the water, I estimated the whole produce at one ounce measure of fixed air.

Lest some mistake should arise from the quantity of air contained in the water itself I at the same time expelled the air by the heat of boiling water from a phial of the same size, filled with the same water, and an equal quantity of oil of vitriol; and I found the quantity of air expelled from it to be quite inconsiderable. In reality, I found after this process, not more than a quarter of an ounce measure of air that was not affected by lime water. There was also not more than half an ounce measure of fixed air collected; so that I allowed half of the fixed air to have escaped the lime water, in order to make the whole equal to one ounce measure.

Then, in order to estimate the quantity of air that I could compleatly phlogisticate by the respiration of two minutes, I breathed, through a glass syphon, the air contained in a receiver that held 200 ounces of water, the receiver being inverted in a trough of water. So long I found that I could breathe the air
contained

contained in this receiver with tolerable ease; and examining the quality of it afterwards, by means of nitrous air, I found the measures of the test to be 1.5. I repeated the experiment with the same event. At the same time using the same nitrous air and common air, the measures of the test were 1.26.

Taking this number from 2.0, the whole quantity of common air that had disappeared was 0.74; but in the air that I had breathed the quantity that had disappeared was 0.5; which, taken from 0.74, leaves 0.24 for the measure of what this respired air was short of compleat phlogistication. Using therefore the following proportion, as 0.74 is to 2.0, so is 0.24 to 64.8. This I therefore conclude to be the quantity of air which I could have compleatly phlogisticated by the respiration of two minutes. It amounts therefore to 32.4 ounce measures, or about a quart in a minute; whereas it is generally supposed that we phlogisticate, or as it has usually been termed, that *we consume* a gallon of air in a minute. And if by consuming be meant

reducing the air to a state in which a candle will not burn in it, the estimate will be pretty near the truth.

If this process can be depended upon, and if the fixed air produced by respiration be precipitated from the common air, it may be concluded that fixed air makes about a sixty fifth part of the mass of common air, which is about the same proportion that the permanent residuum bears to any quantity of fixed air. For beyond that proportion, it is not possible to make water imbibe fixed air.

SECTION XI.

Observations on putrefaction, with a view to ascertain the origin of the fixed air discovered by it.

THE uncertainty of the conclusion from the experiments recited in the preceding section, arises chiefly from the quantity of fixed air that may be supposed to escape the lime water through which it is breathed. But I think that I made a pretty liberal allowance by supposing it to be one half

half of the whole, considering how very readily fixed air is absorbed by quick lime in water. If, however, this one sixty fifth part, or even more than double that quantity, be all the fixed air that is discoverable in common air by means of respiration, there must be some other cause of the diminution of air produced by phlogiston, even in this process, besides the precipitation of fixed air. For in a complete phlogistication (for which I made the above calculation) the diminution is nearly one fourth of the whole. And when the diminution of the air is made by putrefaction, not only does it amount to a complete fourth part of the whole, notwithstanding the production of some permanent air from the putrefying substance, but it has, in all respects, the appearance of being produced solely by the mere precipitation of fixed air.

The following experiments were made with a view to this very circumstance, and they were made with as much attention as I was capable of giving to them. My reader will find an experiment of the same kind in

my first volume on the subject of air; but though it is there very faithfully reported, yet as I was at that time but a novice in these processes, I chose to go over it again, taking it on a larger scale, and with some precautions which I did not then attend to.

Notwithstanding what I had observed before, I had some suspicion that the diminution of air, after the process had been long continued in quick-silver, would not be quite so great as when it was made in water; and when consequently that part of the air which had disappeared had an opportunity of being immediately separated from the rest, and imbibed by the water, with which it was in immediate contact.

Having this circumstance particularly in view, on the 13th of March 1780, I took two dead mice, of about equal size, and put them into two separate cups, under different jars of common air, of very nearly equal capacities, one of them containing 155 ounces of water, standing in quick-silver, and the other 160 ounces, standing in water.

Leaving

Leaving them in the country to the care of a person who supplied the vessels in which they stood occasionally with water or quick-silver, I went to London, and after my return, in the beginning of August, I found, by marking the vessels, and measuring them afterwards, that the air in the vessel which had stood in water was reduced to 140 ounce measures; and on the 28th of August it was reduced to 135, but after standing a fortnight longer, it was not sensibly diminished any farther. The air in the vessel which had stood in quick-silver was not sensibly diminished at all.

Admitting lime water to this vessel, it presently became turbid; but this being a slow diminution I removed the vessel after some days to a trough of water, and then found that the air contained in it made lime water exceedingly turbid; and agitating this air in small portions it was presently reduced to 125 ounce measures; so that all the quantity diminished seems to have been fixed air, making lime water turbid, and being absorbed by water in the very same manner.

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The air in the vessel which had stood in water, notwithstanding the opportunity there was for the fixed air deposited by it being readily absorbed, made lime water very turbid; and by agitation in small portions this air was reduced to 130 ounce measures. Upon the whole then it appears, that the diminution in both of these cases was nearly equal, viz. a little more than one fifth.

In these experiments the two mice were thoroughly putrefied, and indeed quite dissolved, and no doubt had yielded all the air they were capable of yielding. But if the experiments on the putrefaction of mice in quicksilver recited above be compared with these, it will be found that the addition of fixed air, or air of any other kind, from the putrefied mice was quite inconsiderable, viz. an ounce measure and half of fixed air, and half an ounce measure of inflammable from each.

It is true that mice putrefying in *water* yield perhaps more fixed air than in this proportion; but here they putrefied in *air* only. And that a very inconsiderable quantity is produced

produced in these circumstances, is evident from there being little or no increase of the air when it is confined by *quick-silver*, which could not imbibe fixed air, if any had been discharged from the putrefying mice.

If we were to estimate the proportion that the fixed air naturally contained in the atmosphere bears to its other constituent parts, from the data supplied by these experiments, it must be considered as not less than one fifth of the whole mass; and yet it is equally certain that in other phlogistic processes, the diminution has been quite as much without any appearance of fixed air. That there is no appearance of any when iron filings and brimstone are made use of, I observed before, and endeavoured to account for; but I am not able to account for it in the following experiments, at least in one of them.

Having, for a purpose that will be mentioned hereafter, introduced a quantity of nitrous air to the usual proportion of common air, confined by quicksilver, I observed that the diminution was complete without any admission of water; and lime water being afterwards

afterwards admitted to this air was not made turbid by it. Possibly, however, the fixed air in this case might unite with the saline substance formed by the union of the nitrous acid and quick-silver; as all the saline substances on which I have yet made the experiments do yield some fixed air. But I cannot imagine what could have become of the fixed air, if there be any deposited by phlogistication from the common air, in the following case.

Air is as capable of being phlogisticated and diminished by inflammable air, as by nitrous air; and I found the same proportion of it sufficient for the purpose; but inflammable air must be ignited before it can part with its phlogiston to common air. I made the experiment repeatedly in quick-silver, by means of electric explosions, and observed that the whole diminution was always produced instantaneously, and even lime water admitted to the air immediately afterwards did not make it in the least turbid, or produce any farther diminution. This result therefore was the very reverse of the diminution of
air

air by respiration, and especially by putrefaction.

I not only repeated this experiment several times, and with as little loss of time as possible transferred the diminished air to lime water; but I made the diminution itself in lime water, without producing any turbid appearance whatever.

I also made repeated diminutions of common air by means of inflammable air and the electric spark over *water*, in order to discover what it was that the air lost in phlogistication, and what becomes of that part which had disappeared; suspecting that it might have been imbibed by the water, so as to be capable of being reproduced in the form of air by the application of heat. But the result was exactly similar to what I had observed when the diminution was made by the same means over quick-silver. For in this case also the whole of the diminution took place at once, and no fixed air was afterwards found in the water.

To make this experiment to the most advantage, I mixed a large quantity of air, one
third

third inflammable, and two thirds common, and then took of it such a quantity as I found by experience I could easily manage at one time; and putting it into one of Mr. Nairne's inflammable air pistols, previously filled with water, I carefully closed the orifice, by tying round the mouth of it a moistened bladder, out of which all the air was very carefully pressed.

When, in these circumstances, the air within the pistol was fired by means of the electric explosion, the first effect was, that the expanded air was instantly thrown with great violence into the bladder, together with the water contained in the cavity of the pistol; but immediately after, the air, the water, and even the greatest part of the bladder itself, were forced by the external air in the pistol; the air contained in the pistol being now reduced by phlogistication into less space than it had occupied before. I then carefully withdrew the bladder, and preserving the same water, repeated the same experiment with it, till I had decomposed so
much

much air, that even the quantity that had disappeared of the common air, exclusive of the inflammable air, must have been considerably more in bulk than the water. Then putting this water into a phial, I endeavoured to expel air from it by heat; but I found no more in it, than such water usually contains, which was quite inconsiderable; for it was rain water which had been boiled not long before, for the purpose of expelling all its air. The water indeed had a turbid appearance, but this was probably occasioned by the bladder. Had it come from the air, and especially fixed air, it would have been driven out by boiling.

SECTION XII.

*Of changes produced in various kinds of air
by the same processes.*

THAT phlogiston exhaling from vegetable or animal substances should sensibly affect common air, or dephlogisticated air, which contain little or no phlogiston, and have a strong affinity with it, is far from being extraordinary; but that the same substances which phlogistificate common air, or dephlogisticated air, should likewise affect nitrous air, or inflammable air, which already contain phlogiston (and, as it should seem, to a complete saturation) is a fact that I cannot well explain. This, however, I have observed to be the case with liver of sulphur, iron filings and brimstone, and various other substances on nitrous air.

Inflammable air I have not observed to have any impression made upon it by these substances, any more than by the electric spark, at least in the temperature of the atmosphere; though, in consequence of simple confinement

ment by water, it has at length, in several instances, lost its inflammability, and, like nitrous air in the preceding circumstances, has become mere phlogisticated air. I have some suspicion, however, that inflammable air may be decomposed by all the same substances that decompose nitrous air, if more *heat*, or more *time*, be given to the process; and perhaps what I considered as *pure water* might, in time at least, have got an impregnation of something that might affect the inflammable air, in those cases in which I found it reduced to the state of phlogisticated air. These suspicions I have been led to in consequence of observing that *urine* had this effect, both on nitrous and inflammable air; an observation which I made accidentally, in the course of exposing a great variety of substances to the sun during the summer of 1779.

Among other things, I had filled a glass tube, about half an inch in diameter, and three feet and a half long, with urine, and had placed it inverted in a basin of the same. In this situation it was kept several months,

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when

when it yielded at first a small quantity of air, all of which was afterwards absorbed. After which I perceived crystals to be formed in several places of the inside of the tube, and the urine, from being of a pretty high colour, became very pale.

Seeing no farther change in the urine, and having observed its power of emitting and absorbing air, I exposed to its influence all the kinds of air that could be confined by it, in separate six ounce phials, of which the air of each kind occupied about one fourth, the remainder of the phial containing this old pale urine; and the phials were inverted in basins of the same, and, as it evaporated, were supplied from time to time with more urine.

Things were disposed in this manner on the 27th of July, and I observed that there was no immediate change, either in the inflammable or the phlogisticated air; but the surface of the urine in contact with the common air, dephlogisticated air, and nitrous air, from a pale straw colour, presently became of a deep brown, and especially next the dephlogisticated air. The next morning
the

the colour of the urine in contact with the dephlogistified air was almost black, and extended through the whole phial. But in the phial in which the common air was confined, the brown colour extended only a little way within the body of the urine. Under the nitrous air the urine was pretty uniformly brown, but not so much so as under the dephlogistified air. A little both of the dephlogistified and nitrous air was absorbed, and nearly an equal quantity of each.

The dephlogistified air having diminished very fast, and becoming thoroughly phlogistified, I introduced more of the same air into the phial; and I let all the phials stand in this situation till the 22d of July following, when I was obliged to put an end to the process, and I then noted the following appearances. The common air was diminished about one fourth, and was thoroughly phlogistified; the urine being of an orange colour, but not very deep. The dephlogistified air, having been renewed, was not completely phlogistified, but was nearly so. The nitrous air was diminished one

half, had changed the urine black, extinguished a candle, and did not affect common air at all, so that it was mere phlogisticated air. But what is remarkable, this phlogisticated air was in a much greater proportion than is generally procured from nitrous air. This effect I ascribe to the *length of time* that the process took up, and I shall hereafter produce another remarkable fact in confirmation of this opinion.

The inflammable air was diminished to about one eighth of its bulk, and was still slightly inflammable. With more time I doubt not this air would have lost all its inflammability. The urine in this phial was of a very pale colour.

The phlogisticated air alone remained quite unaffected during the whole process, and the urine was of the same colour with that under the common air (*viz.*) a light orange; but this change probably came from that part of the urine, which had been exposed to the common air in the cup, and had gradually extended itself to the urine in the phial.

If the diminution of all these kinds of air was owing to phlogiston, it may be inferred that this principle in the phlogisticated air has a firmer union with its base than it has in nitrous or inflammable air, being less capable of either receiving more, or of parting with what it has got.

But perhaps the most puzzling circumstance in this process is, that the diminution of both the dephlogisticated and nitrous air should be accompanied with the same change of colour in the urine exposed to them. A similar change of colour in a solution of copperas, I thought was owing to the phlogiston deposited from the decomposed nitrous air. But if this was the cause of the similar change of colour in this case, how came the same change to take place in consequence of the diminution of dephlogisticated air, this diminution being, no doubt, owing to its receiving phlogiston from the urine? It can hardly be that the same change should take place in the colour of the urine whether it contains more or less phlogiston than it naturally has. Perhaps it may be something common to

the constitution both of nitrous and dephlogisticated air, and not phlogiston, that, when they are decomposed, is precipitated, and produces this change of colour in the urine.

On the 17th of July 1779, I exposed all the same kinds of air to a quantity of water in which I had dissolved as much *bay salt*, as made it equal to the saltiness of the sea; having been led to do so, in consequence of having observed certain appearances in water of this degree of saltiness, exposed along with the urine above mentioned to the sun. But the results in this case were not very remarkable.

On the 28th of July I observed that the water of the phial in which the nitrous air was contained was become of a darker colour, and that about a tenth of the air was absorbed. In the other phials I perceived no change at all. On the 24th of July 1780, when I put an end to the experiment, the nitrous air was diminished about one half, and had lost so much of its virtue, that when it was mixed with an equal quantity
of

of common air, the measures of the test were 1.4. The dephlogisticated air was a little diminished, and altered in proportion; but the common air, the inflammable air, and the phlogisticated air, were not sensibly changed at all. The result of this process will be easily perceived to be exactly similar to that of the former, but the *cause of diminution* seems not to have acted so powerfully in this case as in that.

That *pure water* does not injure air, either in the form of vapour, or otherwise, is I think sufficiently evident from observing that the air within one of the glass tubes, hermetically sealed, in which a quantity of water had been exposed several months, in a sand heat, was not at all injured by it; and the trial was made more than a year after an end had been put to the experiments with the sand heat. Nitrous air also remained unchanged in the same circumstances.

SECTION XIII.

Of the respiration of fishes.

I HAD formerly found that fishes injure the air contained in solution in the water in which they live, vol. III. p. 342; the water in which they had been confined appearing to contain air of a worse quality, than it did before they were put into it. I had also before observed the effect of water impregnated with fixed air, and with nitrous air, on fishes put into it. I have since repeated all these experiments with an attention to more circumstances; and they both confirm and extend my former general conclusions.

Having at hand some water from the Hot-well at Bristol, which I had found to contain air in a state of great purity, I completely filled a large phial with it, and I put into it a few very small fishes, which I had provided for the purpose of these and other experiments. They were minnows, and other small fishes, about two inches in length. In
this

this water they were confined without any access of common air till they died.

After this I took equal quantities of the water in which the fishes had died, and of that out of which it had been taken, when they were confined in it; and I expelled from both all the air which they would yield. That from the water in which no fishes had been put, exceeded in quantity that from the water in which they had been confined in the proportion of three to two; and examining the quality of both these quantities of air, by the test of nitrous air, the former exceeded the latter in a still greater proportion. The air from the water in which no fishes had been confined was about the standard of common air, but that which had been contaminated by the respiration, as I may say, of the fishes, though not thoroughly phlogisticated, was something worse than air in which a candle just goes out. I should probably have found it still worse than this, if I had expelled and examined the air immediately; but the water remained in an open vessel all night before I made the experiment upon it.

From

From this experiment it may be concluded with certainty, that air contained in water, in an unelastic state, is as necessary to the life of fishes, as air in an elastic state is to that of land animals. It is not properly *water* that receives the phlogiston discharged from the fishes, but the *air* that is incorporated with it. And this may possibly be the reason of the attraction which, in many of my experiments, there appears to be between phlogiston and water; whereas it has been an opinion universally received among chemists, that water has no affinity whatever with phlogiston.

From this experiment I had no doubt, but that putting fishes into water impregnated with air that was thoroughly phlogisticated, would be injurious, if not fatal to them, as much as the same kind of air, in an elastic state, is to land animals; and this was verified by the following experiments; from which, however, it appears that fishes, like insects, and some other exanguious animals, can live a considerable time without any thing equivalent to respiration.

ration. What limits that time has, may in some measure appear from these observations.

I began with water that contained, as far as we are able to discover, no air at all. For it was rain water, that had been recently boiled a considerable time. The vessel contained about three pints of it; and into this, without admitting any air at all, I put nine of the small fishes abovementioned, and they lived in it between three and four hours. This experiment resembles the putting of frogs and serpents into a vacuum, only that there was no expansion of air contained in them to swell their bodies in this case.

Taking the same water, which as I observed, contained little or no air, I made it imbibe as much as I could of a quantity that had been phlogisticated with iron filings and brimstone, six months before. Of this, however, the water would take but very little. Into a pint of this water, thus imperfectly impregnated, I put two of the fishes, and they lived in it near an hour. The result was the same when I impregnated an equal quantity of the same water with inflammable

ble air. For in this case also the two fishes lived about an hour. This experiment resembled the putting of mice, and other land animals, into phlogisticated or inflammable air, which is known to be fatal to them, but more suddenly than this water was to the fishes, owing, I suppose, to its imperfect impregnation.

When I impregnated water with nitrous air, on a former occasion (see vol. II. p. 231) I observed that fishes put into it were immediately seized with convulsions, and died presently; just as they did in water impregnated with fixed air. But though at that time I took all the care I could to prevent the decomposition of the nitrous air, that remained after the operation, filling the phial in which the process was made with fresh water by means of a funnel, &c. still a decomposition of some small part of it would necessarily be made, before I could possibly slip the funnel into the neck of the phial. To prevent this, I now introduced the fishes into the vessel in which I had impregnated the water while it remained inverted in the basin, the remainder of the nitrous air
not

not imbibed by the water still resting upon it. The phial I used contained something more than a pint, and the nitrous air occupied about one fourth of it.

Into this vessel, thus prepared, I introduced two of my small fishes, and they continued very quiet, without being seized with any convulsions, ten minutes, or a quarter of an hour, before they died. The cause of the convulsions, therefore, in the former experiment, must have been not the *nitrous air*, properly speaking, but the *nitrous acid*, though in so very small a quantity, diffused in the water, and acting like the fixed air (which is only another kind of acid) in the water impregnated with it. Whereas in this experiment the fishes were no otherwise affected than they were in the water impregnated with phlogisticated or inflammable air, except that the water imbibed much more of the nitrous air, and on that account was sooner fatal to them.

SECTION XIV.

Of the production and constitution of dephlogisticated air.

AT the time of my last publication, the readiest method I had of procuring this dephlogisticated air was by moistening minium with spirit of nitre, and then exposing it to a red heat in a gun barrel. But being now in possession of small furnaces, and being a little more conversant than I then was in the common operations of chemistry (though I still have little to boast of in that respect) I find Mr. Scheele's method of procuring it (and indeed that in which it will be found that I myself first of all procured it, but without knowing what I had got) viz. from *nitre alone*, much preferable.

For this purpose an earthen retort, and a reverberatory furnace (for which however one black pot or hessian crucible inverted over another answers very well) are necessary. This is also the cheapest method I know

know of procuring this air. For every ounce of nitre will yield at least a hundred ounce measures of very pure dephlogisticated air; and the fire may be so regulated, that the production of air shall be more equable, and the process more manageable than in the method I had been used to. I therefore cannot help recommending it to all persons conversant in these experiments, and who have any thing of a reverberatory furnace; though I doubt not but that covering the retort with coals, in a common fire, would answer very well.

Having before ascertained the production of dephlogisticated air from various substances containing *vitriolic acid*, without any mixture of nitrous acid, and among others from *alum*, I have since made one experiment in order to ascertain the quantity of this pure air that a given quantity of alum could be made to yield. For this purpose I put 1 oz. 14 dwts. of calcined alum into an earthen retort, and by means of a reverboratory furnace I extracted from it one hundred ounce measures of air, a small part of which was
fixed

fixed air, and the rest so pure, that with two equal quantities of nitrous air, the measures of the test were 1.0.

The water in which this air was received was strongly impregnated with vitriolic acid air. This air containing much phlogiston, and in a state in which it can be imparted to air, was, no doubt, the reason why the air in this case was not so pure as that which is obtained from nitre. Otherwise this would be the cheapest and best method of procuring dephlogisticated air.

Collecting what remained of the alum it weighed 15 dwts. and still had the taste of alum, though not very strongly. More heat would probably have expelled all the acid, and consequently would have procured more dephlogisticated air. Also had the air been quite pure, it would, no doubt, have been much more in quantity. If the weight of the alum, and of the residuum after this process be compared, it will be found that this one hundred ounce measures of air occasioned the loss of only 19 dwts. that is, not quite an ounce of the alum,
and

and since the 100 ounce measures of dephlogistified air would weigh about 2 dwts. 18 grains, the weight of the vitriolic acid air, with which the water was impregnated, may be estimated at 16 dwts. 6 grains.

Another time I got 60 ounce measures of air from an ounce of alum, which is in about the same proportion as in the former experiment. But it had been so well calcined previous to this process, that some of the air had probably been expelled in that operation; and still what remained tasted very sensibly of alum. This air being examined, the measures of the test, with two equal quantities of nitrous air, were 1.4. There was hardly the least sensible quantity of fixed air produced.

It is easy to conceive, that till any substance be completely dephlogistified, it cannot yield dephlogistified air; and it is something remarkable, that a *red colour* should be the criterion of dephlogistification, both in the calx of iron, and of mercury. Accordingly, when mercury is dissolved in spirit of nitre, the produce is pure nitrous
L air,

air, not only during the solution itself, but also during the application of heat to the yellow concrete mass, that is formed by the evaporation of the solution ; and no dephlogisticated air is produced till the red precipitate is completely formed. But the action of heat upon this red substance is always followed by the production of pure air, as much as from the precipitate per se.

It is also evident from this experiment, that the air produced in it does not come from the atmosphere, which has been conjectured with respect to some of the processes for procuring dephlogisticated air ; but must have been contained in the ingredients, viz. spirit of nitre and mercury. I have no doubt but that both of them contribute to it. That spirit of nitre, in a very great proportion, enters into the composition of dephlogisticated air, is evident from the production of it from nitre only ; and that some *earth* is likewise requisite for the purpose, is hardly less evident, from the loss in the weight of mercury revived from red precipitate, which I have never found to be less
than

than one twentieth part. This, however, is a small matter; and the conclusion from it seems to be, that dephlogisticated air consists of about nineteen parts acid, and a twentieth part earth. I do not say nitrous acid always, because the vitriolic is, in many cases, no less capable of furnishing it; but of some acid principle common to them both.

In the rapid production of all kinds of air from earthy materials, I have frequently observed that there is a quantity of superfluous *white matter* deposited in the cold water in which it is received. This earth seems to have been held in solution in the air while it was hot, because it was then quite transparent, and did not become turbid till it was cool; and this is one reason why I think that an earth is the proper basis of all such kinds of air. For if some earth be certainly held in a proper solution, so as to make a constituent part of the air while hot, as its transparency seems to prove, and it be only deposited by *cold*, some of the earth must be retained by it in every degree of heat, and therefore in the temperature of the atmos-

phere. And perhaps no degree of cold can deprive it of all the earth that it contains. If it should, I should imagine that, as nothing but the *acid principle* would remain, it would then, like any other *acid air*, become liable to be immediately absorbed by water.

This earthy matter when incorporated in the air, I should imagine to be then *the same thing*, from whatever substance the air had been produced, being then divested of every thing that was peculiar to the substance from which it had been expelled; just as the *acid*, in the composition of dephlogisticated air, is probably the same thing, whether the air had been produced from materials containing spirit of nitre, or oil of vitriol. If this reasoning be true, we shall be in possession of a method of obtaining a truly *primitive earth*, or an *earthy principle* common to all earths, and all metallic calces whatsoever, since dephlogisticated air may, as I have sufficiently shewn, be produced from them all. The following observations, however, may perhaps lead to a contrary conclusion, or, that earth deposited from dephlogisticated air produced

duced from different materials, has not, in all respects, the same properties; though I am inclined to think that, if the trials could be made quite unexceptionably, they would favour the former hypothesis.

Having collected some of the white powder diffused through a quantity of dephlogisticated air, procured from minium and spirit of nitre, I observed that when it was dry, it was of a grey colour, and that it was not, at least immediately, affected by spirit of salt. When it was heated in a glass tube, by means of the flame of a candle and a blowpipe, it fumed copiously, and covered the inside of the tube with a white substance; that which was not sublimed becoming black. When it was laid upon a red hot iron, it smoked very much, and became of a brown colour. But in none of its forms was it quickly affected by spirit of salt, though after twelve hours this acid did acquire an orange colour, both from the black and the brown matter.

At the same time I had by me a quantity of white matter which, as I believe, had been collected in a similar manner, when I pro-

cured dephlogisticated air from *red precipitate*; but having lost the label, I cannot be absolutely certain. This matter was perfectly white when dry, and bore a red heat without any sensible change, nor was it affected by spirit of salt. Willing to procure some of this matter, that I might be sure was from mercury, I made a quantity of red precipitate; and having put it into a gun barrel, I urged it with as great a heat as I could excite in a common fire with a pair of double bellows; but, to my surprize and disappointment, all the air came over quite transparent. I had before made this process in small glass retorts; but I had no suspicion but that I should have had the same result with the gun barrel. It is a tedious process to procure much of this white matter; but when I am a little more at leisure I may possibly repeat my attempts.

It has been imagined by Mr. Lavoisier, that the calces of metals attract dephlogisticated air from the atmosphere, in the process of calcination, and consequently that the calces exposed to heat only throw out the
very

very air which they had before imbibed. This is particularly his idea of the origin of that dephlogisticated air which is procured from precipitate per se, which is made by exposing mercury a long time to a moderate heat in glass vessels, from which the common air is not excluded. But there is no occasion to suppose that *all* the materials which constitute dephlogisticated air are gained from the atmosphere. For all that mercury wants to make it capable of yielding this air, is the loss of its own phlogiston and the acquisition of some acid principle, which appears to be common to the nitrous and vitriolic, if not to other acids also. This acid, therefore, it seems to get from the atmosphere, at the same time that it parts with its own phlogiston to it.

But since, according to the theory of Dr. Crawford (which I do not pretend to have sufficiently considered) air, in parting with its phlogiston, acquires the *principle of heat*, are not these two things, viz. *heat* and *pure acid* the same; which is nearly the idea of Mr. Sheele? But as this principle of heat does not, in any other case, appear to assume

the form of air, and has not been found to have *weight*, which all acids, and dephlogisticated air also, have; it seems to be more probable, that the calx in parting with this phlogiston, takes from the air two distinct principles at the same time, viz. that of heat (if Dr. Crawford's theory be true) and this acid.

Much remains to be observed, and to be done, on this subject. In the mean time I shall only recite an experiment which proves that precipitate per se, or the true calx of mercury, is much more easily procured in dephlogisticated than in common air, and probably not at all in phlogisticated air; this air not being capable of taking any phlogiston from mercury, without which the calx cannot be formed.

I exposed equal quantities of the same quick-silver, in equal glass tubes, of about two feet and a half long, and an inch and a half in diameter, but narrower towards the top, to the same heat, for one day, one of the tubes containing phlogisticated, and the other dephlogisticated air, both hermetically sealed. In the result, the mercury in the tube
containing

containing the dephlogisticated air was completely covered with a coating of precipitate per se; but the mercury in the other tube was not sensibly altered. When the process had been resumed, and continued four days, I opened the tubes, and found the dephlogisticated air something worse than it had been, but by no means so much so as I had expected; but the phlogisticated air was not at all altered. The quantity of precipitate in the dephlogisticated air was trifling.

I then repeated the same experiments with common air, but in two days no precipitate was formed. With more time there probably would have been some. But this was sufficient for my purpose, viz. to ascertain the difference that would be produced by different kinds of air in this process, according to the quantity of phlogiston which they contained.

In the former experiment the dephlogisticated air was confined in the same tube with the mercury, but afterwards I exposed a quantity of quick-silver to a sand heat in a glass retort, which had a communication, by
means

means of a glass tube, with a large reservoir of dephlogisticated air. But though I continued this process several days, I did not find that consumption of dephlogisticated air which I expected. I had little doubt, however, but that, by an attention to these hints, the *precipitate per se* may be made in much less time, and with much less expence, than it now is.

Lastly, I would observe, as it has some little connection with the other subjects in this section, that the dephlogisticated air which I have mentioned, vol. IV. p. 253, as having been confined by quick-silver, in a phial filled with iron nails, from the 13th of April 1778, being examined on the 20th of July 1780, was not diminished any farther than it is there observed to have been in 1779. It was also a little worse than it had been when it was put into the phial, and the nails were very clean and without rust.

S E C T I O N XV.

Of the respiration of dephlogisticated air.

SOME of my friends have expressed a little doubt about the certainty of the test of nitrous air, as a measure of the wholesomeness of respirable air in general, and of dephlogisticated air in particular. To this I can only say, that every thing I have yet observed leads me to depend upon the accuracy of this test, with respect to dephlogisticated as well as common air; and, according to what I should think to be the fairest method of computation, dephlogisticated air serves even longer for respiration than from this method of examining it I should have conjectured *a priori*.

The most natural method, as I should think, for estimating the purity of air, and thereby judging of the *time* that any given quantity of it would suffice for the purpose of respiration, would be to find the quantity of phlogiston that is required to saturate it, or
which

which comes to the same thing, the quantity of nitrous air that is required to bring it to the state of perfectly phlogisticated air. But a mouse will live much longer in a given quantity of dephlogisticated air, than in this proportion, with respect to common air; owing, I suppose, to the animal not throwing out equal quantities of phlogiston in equal times, but much less at the last, when the vital powers are languid, than at the first.

I have a glass vessel which I have made use of in all my experiments with mice, from the beginning of my researches into this subject, a considerable time before I had discovered nitrous air. In this vessel, which was the top of a tall beer glass, and which holds about two ounce measures of air when a mouse of a middle size is confined in it, I never knew any mouse to live longer than half an hour, and in general they have not survived twenty minutes. Supposing, however, the full time for a mouse's breathing the common air contained in this vessel to be half an hour, I should not have expected that

that a species of air which required only *four times* as much nitrous air to saturate it, would suffice for the respiration of the same animal more than four times as long; and therefore that in this vessel of dephlogisticated air, which at a medium requires about that proportion of nitrous air to saturate it, a mouse might live about two hours. But I believe that mice in general will live considerably longer in that quantity of dephlogisticated air.

I lately put a young mouse into that very vessel, filled with dephlogisticated air, so pure that, with two equal quantities of nitrous air, the measures of the test were, 0.55. It continued there near three full hours; and being taken out alive, the air was found to be so far from being phlogisticated, that it was still considerably better than common air; for, with an equal quantity of nitrous air, the measures of the test were 1.05. Perhaps this mouse being languid, in consequence of its confinement, did not phlogisticate the air so fast as it would have done had it been more vigorous; But then this may in general be expected

expected to be the case with all mice, in the same situation.

My friend Dr. Ingenhoufz has announced what he thought to be a very valuable discovery, of the Abbe Fontana's, with respect to the breathing of dephlogistified air; and had there been no mistake in the business, it would have been a discovery of the very first magnitude. It is a method of making dephlogistified air serve thirty times longer for respiration than when it is breathed in the common way, so that a pound of nitre would yield dephlogistified air sufficient for the respiration of a man a whole day.

“The Abbe Fontana,” he says, p. 46,
“found that an animal breathing in either
“common or dephlogistified air, renders it
“unfit for respiration by communicating to
“it a considerable proportion of fixed air,
“which is generated in our body, and thrown
“out by the lungs as excrementitious. This
“fixed air is easily absorbed by shaking it in
“common water, but infinitely more readily
“by the contact with quick lime water.”

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Then, after describing a method of breathing this air, which is by introducing a syphon through the water into the vessel containing air, he says, that the discovery consists in using *lime water* instead of common water. "The Abbe," he says, p. 48, "found
" that the dephlogisticated air being, after
" each respiration, purified again by the lime
" water, will remain good about thirty times
" as long as it would when breathed in the
" ordinary way, and that thus the quantity
" of dephlogisticated air necessary for one
" minute will now serve for breathing during
" half an hour, and thus the expence
" will be thirty times less."

This language supposes that the Abbe had not only *reasoned* upon the case, but that he had also *verified* his reasoning by actual experiment; because it is said that he *found* it to be so. On the contrary, I can neither find any such thing in fact, nor the least colour for the expectation of it in reasoning; there being no advantage whatever in breathing dephlogisticated air in the manner that Dr. Ingenhousz describes. And his hypothesis

thesis concerning the nature of the injury that is done to air by respiration is manifestly erroneous. For the precipitation that is made of fixed air is nothing more than a *circumstance* attending the respiration of common or dephlogisticated air, the proper effect of that animal process being, as I think I have fully demonstrated, the *phlogistication* of the air; and therefore, though the precipitated fixed air be absorbed ever so readily, the remaining air will be but very little the better for it. For if we were to mix much more than that proportion of fixed air with the air that we breathe, we should not perceive it to be at all inconvenient to us.

It was but reasonable, however, that the assertion of so eminent a philosopher, and the assertion of a *fact*, should be tried by fact. I therefore took a young mouse, and put it into three ounce measures and an half of dephlogisticated air, so pure that, with two equal quantities of nitrous air, the measures of the test were 0.25, confined by lime water. In these circumstances the mouse lived three hours and a half; and though it was
taken

taken out alive, it died presently after. The air, however, was not thoroughly phlogisticated; for, with an equal quantity of nitrous air, the measures of the test were 1.35. This, though no decisive experiment, shewed that the approximation to complete phlogistication was nearly the same as in the experiment recited above, in which the air was not confined by lime water.

But to make the experiment in the most unexceptionable manner that I could contrive, I, in the next place, got two mice, of nearly equal size, and put them into exactly equal quantities, viz. about five ounce measures, of the same dephlogisticated air (the measure of its purity, with two equal quantities of nitrous air, being 0.24) in nearly equal and similar glass jars, one standing in lime water, and the other in common water. Both the mice continued in this situation something more than two hours and an half, after which the air which had been confined by lime water appeared to be reduced in the proportion of 9 to $5\frac{1}{4}$ the measures of the test being 0.96; and the air

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which

which had not been confined by lime water was diminished in the proportion of 9 to $6\frac{3}{4}$, the measures of the test being 0.98. Both the mice, though kept pretty warm, laboured alike with a difficulty of respiration, some time before I put an end to experiment. In the course of it I agitated the lime water a little now and then, in order to make it absorb the fixed air the better, by admitting fresh lime water to the air that had been respired.

It appears from this experiment, that the air confined by lime water was both diminished and phlogisticated exactly like that which had been confined by common water, by the respiration of mice of equal size, in the same time. The diminution indeed was, at first, a small matter greater in the air confined by the lime water; because the common water did not imbibe the fixed air so readily; but this made no apparent difference with respect to the mice, and the next day the two portions of air were found to be as nearly as possible of the same dimensions, and of the same degree of purity.

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In the preceding experiments, and several others which I made about the same time, I found that mice would not live in dephlogistified air till they had completely phlogistified it, though they lived longer in it than, in proportion to its purity, with respect to common air ; and for this I cannot assign any sufficient reason. I had once imagined that this was owing to my being obliged to make the mice pass through a quantity of water, by which the air was confined ; but I put a mouse through the same water into a quantity of common air, and it lived in it till it was thoroughly phlogistified. This may deserve a farther investigation. I should have put other mice into what remained of the dephlogistified air.

SECTION XVI.

Observations relating to fixed air.

MOST saline substances, I believe, contain more or less fixed air; and it may be worth while to examine what *quantity* of it may be extracted from each of them, and also the quality of the residuum, which I find to differ considerably in different cases. But this may depend, in a great measure, upon the state of the water in which the experiments are made, which will take more or less of phlogiston from such residuums. A few observations that I have had occasion to make of this kind may be just worth noticing.

Both *vitriolated tartar*, and *Glauber salt*, which I have often occasion to make in the course of my experiments, I find contain fixed air. Dissolving a quantity of vitriolated tartar, which was formed in making spirit of nitre, and collecting the air that came from it, I found one twelfth of it to be

be fixed air; and with an equal quantity of nitrous air, the measures of the test for the remainder were 1.3. At another time I filled the retort in which the salt was contained with boiled pump water, and then I found no fixed air in it; having, I suppose, been absorbed by the water, and the measures of the test for the remainder were 1.46. Again I dissolved a quantity of this salt in pump water, and then found one fourth of the whole to be fixed air; the pump water itself containing a good deal, and the measures for the residuum were 1.44.

I also dissolved a quantity of Glauber salt, which remained from the process for making spirit of salt, and I found the residuum of the fixed air to be sensibly worse than common air.

In dissolving alum, in order to get some earth of alum, I observed that air was discharged from it. This I collected, and found it to contain very little fixed air, and the measures of the test for the residuum were 1.12. At another time I had the same result,

but the air was not quite so good, though purer than common air.

Precipitating a solution of alum with potash, I caught the fixed air, which was discharged in great abundance; and examining the residuum, found it to be better than common air, in the proportion of 1.2 to 1.3; the diminution being in that proportion when mixed with equal quantities of nitrous air.

SECTION XVII.

Of the state of air in water.

I HAVE formerly observed that the state of air in water is an object worthy of attention, and that it was not before the time of my last publication that I had ever found air extracted from water to be so good as common air. In general it was in part fixed air, with a residuum that extinguished a candle. I have since, however, frequently found air expelled from water to be much better

better than common air; but I have not yet undertaken any regular course of experiments on the subject; such as examining the same water at different times of the year, with different impregnations, different exposures, &c. which I wish to have done; because I think it possible, that something worth knowing relating to the properties of water, or of air in water, especially respecting phlogiston, and the general state of the atmosphere, may be discovered by this means. Such observations as I have occasionally made I shall here put down.

Boiling always expels more or less of fixed air from water. On the 5th of June 1779, I found my pump water to yield air, one fifth of which was fixed air, and the measures of the test for the residuum were 1.5. The same pump water, which had been boiled some time before, gave air, one seventh of which was fixed air, and the measures of the test for the residuum were 1.4. In general I believe a greater difference than this will be found in these two cases. I do not know that water will attract fixed air from

the atmosphere, at least in the proportion in which it is generally found in pump water, which is probably acquired from calcareous matters first held in solution, and then partially decomposed in it.

Water distilled in glafs, which had been long exposed to the open air, yielded air, of which little or none was fixed air, and with equal quantities of nitrous air, the measures of the test were 1.1.

A quantity of rain water taken from a large tub, which had long stood exposed to the open air yielded one sixtieth of its bulk of air, of which no part was fixed air, and the measures of the test were 1.4. Perhaps the wood of the tub, or some other matter casually falling into it, might have contaminated this air.

A quantity of river water, not very far from the spring, gave one fiftieth of its bulk of air of which the smallest part imaginable was fixed air, and the measures for the rest were 1.05. This air was very pure; but the part of the river from which I took it was nearly stagnant, and very full of water plants.

Lime

Lime water is certain not to contain any fixed air. From a quantity of this water I expelled air so pure that the measures of the test were 1.0. The quantity of air was one fiftieth of its bulk. Upon the whole I am inclined to infer, from all the observations I have hitherto made, that this is about the standard of air contained in water, which has no fixed air, and has been exposed to no influences except those of the common atmosphere, in its usual state. But I propose to make more observations on this subject.

From a spring which was remarkable for its petrefying quality, I expected much fixed air, but I found none; and the air I extracted from it was a little worse than common air. It is plain that, in this case, a boiling heat had not decomposed the lime stone it contained.

I also filled a phial with pump water and pounded lime stone, exposed to the sun from the 28th of May to the 3d of July, when it yielded air so pure, that with two equal quantities of nitrous air, the measures of the test
were

were 1.4. I should have suspected some green vegetable matter in this water, but I could not perceive any. Perhaps some latent, or nascent vegetation might be the cause of this very pure air.

That water parts with its phlogiston to air, and thereby becomes, in some measure, purified, independent of any thing vegetating in it, is I think evident from the following observation. I took some of the Bristol water in which fishes had died, and which then yielded air thoroughly phlogisticated; and having exposed it to the sun from the 28th of May to the 3d of July, I found it to yield a considerable quantity of air; and so pure that, with an equal quantity of nitrous air, the measures of the test were 0.76, and with two equal quantities of nitrous air the measures were 1.18.

SECTION XVIII.

Observations relating to the constitution of nitrous air.

THE subject of nitrous air has made a considerable article in all my former publications relating to air. I shall be obliged to appropriate several sections to the farther observations that I have made upon it in this volume, and there are many things that I still propose to investigate concerning it. I shall begin with such experiments and observations as more immediately relate to the *constitution* of this kind of air.

1. *Of water in the composition of nitrous air.*

That water enters into the composition of nitrous air, is not improbable because it is procured in so great abundance from pure water impregnated with phlogisticated nitrous vapour, and also from its not being procured from copper, and other metals, except in a very diluted solution of the nitrous acid.

acid. But notwithstanding this I have not yet been able to discover any water in the decomposition of nitrous air. The trickling of liquid spirit of nitre down the sides of a glass vessel in which a mixture of nitrous and common air, and more especially of dephlogisticated air is made, over water, is only the moisture, which adhered to the glass, now impregnated with nitrous acid vapour, from the decomposed nitrous air.

When I mix the two kinds of air in quick-silver, I cannot perceive any moisture at all; so that if there be any water in the composition of this air, it must enter into the composition of the *mercurial nitre* formed at this time; and even in this case it might be expected to be discovered before the mercurial nitre was completely formed, which is a considerable time, when the surface of the mercury is not large.

2. *Of the first and subsequent produces of nitrous air.*

In several methods of producing nitrous air the quantity yielded at first is very small,
though

though the solution of the metal from which it is procured is at the same time very rapid. I had therefore some times suspected, that the first produce, even from copper or silver, might differ in some respects from that which came afterwards, as I have observed to be the case very remarkably when this air is procured from tin and zine. But I tried with copper formerly, and with silver lately, and in both cases found that the first flow produce of air diminished common air just as much as that which was produced the most rapidly afterwards.

3. *Of the changes in nitrous air when it is produced from iron.*

When nitrous air is produced from iron, the quality of it, I doubt not, is always the same, though there is a case in which I do not wonder that some persons have been deceived; having got phlogisticated air when they expected nitrous; not considering, that exposure to a large surface of iron decomposes

poses nitrous air, as my former experiments shew; changing at first into a species of air in which a candle will burn, and then into phlogisticated air. This process, however, required a considerable time; but the following experiment shews that this effect may be produced very soon.

I filled an eight ounce phial with small nails, then with water, into which I put a very small quantity of nitrous acid, just enough to make it produce air; and then some was yielded in which a candle went out. Also this acid water poured off from the iron gave a considerable quantity of air, in the heat of boiling water, and this was all phlogisticated air.

Using more spirit of nitre, I observed, that, though the production of air was pretty copious, as was manifest by the bubbles formed at the bottom of the phial, and rising to the top, there was no increase of the whole quantity of air in the phial. Examining the air in this state of the process, I found that it had very little power of diminishing common air,
and

and that a candle burned in it with a vivid flame, which is the intermediate state of this air, before it becomes phlogisticated air. I imagine, therefore, that in all these cases, a proper nitrous air is first produced, and that it is afterwards, by means of the iron to which it is exposed, changed into that species of air in which a candle can burn, and lastly into phlogisticated air, which extinguishes a candle.

4. *Of changes in the colour of liquids by which nitrous air is confined.*

I have formerly observed, that a solution of green vitriol becomes black by the contact of nitrous air. This I find to be a criterion of the presence of a very small quantity of the martial salts in water. When a very little of it has been accidentally formed, in the course of my experiments, and mixed with the water in my trough, I have never failed to discover it by the dark colour of the water in those jars which contained nitrous air.

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This change of colour must, as I observed before, have been produced by the phlogiston of the nitrous air, the nitrous *acid* having no such effect. This was also the case with a solution of copper in nitrous acid; but the change was not from blue to a darker colour, but into *green*.

I filled a jar, about an inch in diameter, and twelve inches long, with that solution of copper in spirit of nitre which remains after making nitrous air, and which is of a beautiful blue colour. Then inverting it in a basin of the same, I introduced to it a quantity of nitrous air. After some time I observed that the air was considerably diminished, and that all the surface of the liquid in contact with the air, to the depth of about a quarter of an inch, was of a beautiful green colour. This air kept diminishing some months, and the green colour of the solution extended two or three inches within the liquid. At last there remained only two sevenths of the original quantity of air; and, examining it in that reduced state, I found it to be mere phlogisticated air. Had it been
examined

examined in the intermediate state, it would, I doubt not, have been found to be of that kind of air in which a candle will burn. The experiment was begun on the 4th of October 1779, and the air was not examined till the 20th of July 1780.

5. *Nitrous air not changed by exposure to water in a sand heat.*

I have observed that nitrous air, confined in flint glass tubes hermetically sealed, was not changed by being exposed even to a red heat, or kept ever so long in hot sand, though inflammable air undergoes a remarkable change in those circumstances. I have since observed that there is no change produced in nitrous air by its being confined along with *water* in this manner. For one of the tubes which had been exposed to the sand heat several months, as related in my last publications, had both water and nitrous air in it. This nitrous air I examined the 24th of November 1779, which was a long time after its

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being

being taken out of the sand heat; but it did not appear to have lost any of its power of diminishing common air.

6. *Of the change in nitrous air from very long keeping in water.*

I have observed that if nitrous air be agitated in water presently after it is made, or indeed after it has been kept some weeks, it will be reduced to a very small quantity, perhaps one twentieth of its original bulk; and it will then be wholesome air. But I find that if it be kept a very *long time*, its constituent principles, as we may say, acquire a much firmer consistence, and that then a remarkably greater proportion of it becomes first phlogisticated air, and then, by agitation in water, wholesome air.

In vol. iv, p. 62, I observed the changes produced in two quarts of nitrous air, one from *iron*, and the other from *copper*, made in November 1773. On removing from Wiltshire to Birmingham, I thought proper
to

to put an end to this process, when I found no farther change in the bulk of these two quantities of air; that which had been produced from iron still occupying two thirds of its original dimensions, and that from copper one half. I agitated in water a portion of each of these quantities of air, without producing any change in their bulk; but they were both considerably improved by it, so that when mixed with equal quantities of fresh nitrous air, the measures of the test were 1.75.

This I consider as a pretty remarkable observation, as it exhibits a change in the constitution of a body depending upon *time* only, and which it is not yet in our power to produce by any other agent or instrument.

SECTION XIX.

Of the mixture of nitrous and common air.

THE business of this section shall be to explain a phenomenon which puzzled me at the time of my last publication in mixing nitrous and common air, and to correct a mistake of the Abbe Fontana, and of Dr. Ingenhoufz, relating to the same subject.

I had observed, vol. iv, p. 77, that if, after having mixed equal quantities of the two kinds of air in my wide jar, I made them ascend very quickly into the long tube on which the measures were marked, the diminution was more than when I made them ascend more slowly; and this difference sometimes amounted to five hundred parts of a measure. This I now conclude arose from what remained of the nitrous air, not decomposed in the mixture, being diminished by passing through so much space of water, which is more exposed to its influence in a
flow

slow than in quick passage. But I own I should not have suspected that nitrous air would have been diminished so very much by being simply poured from one vessel of water into another, if I had not observed it in the following manner.

Having mixed a quantity of air, which I knew to be thoroughly phlogisticated by the putrefaction of fishes, with an equal quantity of nitrous air, I transferred the mixture into my graduated tube ; when, instead of occupying two whole measures, as I had expected, they only occupied 1.95 measures. Suspecting that the five hundred parts of a measure which had disappeared had been absorbed by the water, I poured the air back again into the wide jar ; and transferring it once more into the graduated tube, found it to be only 1.8 measures ; and pouring it about ten times backwards and forwards, without any unnecessary agitation, it was reduced to 1.6. Having stood in water all night, I measured it again the next morning, when I found it to be 1.5 ; and by measuring three times more it was reduced to 1.4.

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I then poured two measures of nitrous air only from the wide jar into the graduated tube, and found that it was diminished even in a greater proportion than the former mixture.

In applying the test of nitrous air, I have lately preferred equal measures of nitrous and of common air, or of any air which may be conjectured *a priori* to be nearly in the state of common air, in order that there might be phlogiston enough to saturate it entirely; and if the remaining nitrous air was not affected by water, this method would be perfectly unexceptionable; and with due precaution, it is not liable to much objection. But the most accurate method would be to use no more nitrous air than the air to be examined is able completely to decompose. But then it cannot be known before hand how much this is. Perhaps, in order to guard against the inconvenience above mentioned, it might be most adviseable, in common cases, that is, when the air to be examined is about the standard of common air, to use something less than an equal quantity of nitrous

trous air, but more than one half, which was the quantity that I first confined myself to.

I would farther observe, that if the simple transferring of nitrous air from one vessel of water to another be liable to some uncertainty, the agitation of it, which the Abbe Fontana uses, must be liable to more. For no person can be quite sure that he makes the agitation exactly alike, especially after any considerable interval of time.

No person has taken more pains in reducing to certainty the method of ascertaining the purity of air by means of nitrous air than the Abbe Fontana, who has likewise given very great attention to the subject of air in general. Dr. Ingenhoufz has had his leave to publish a very particular account of his methods and precautions in this business; and he reasons at large upon the advantage of each part of the process.

Among other advantages of the Abbe's method, and no doubt a very capital one it would be, if he had fallen into no mistake respecting it, is, that it is of no consequence whether the nitrous air he employs be good

or bad; that is, have more or less power of diminishing common air, or, which comes to the same thing, how much mere phlogisticated air be mixed with it.

“ In the method adopted by other philosophers,” he says, p. 173, “ by which a certain proportion of nitrous air is always added at once to a certain quantity of the air under examination, the result is very uncertain, if the nitrous air be not always exactly of the same quality. But in the method of the Abbe Fontana, this article is of no consequence at all. The only difference arising from weak nitrous air in this method, is, that more measures of it are required before the saturation of the air to be examined is completed.”

The circumstance in the Abbe's method on which this pretension is founded, is, that, after putting a certain quantity of nitrous air to the respirable air, he continues to add other equal quantities, till he finds that no farther diminution is produced in the whole; so that, though there might not be phlogiston enough in the first quantity of nitrous air,
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there would be in the second, or third, &c. Then he deducts from the number of all the measures that he had put together, consisting of both respirable and nitrous air, what he finds remaining in his vessel, and by this number he judges of the purity of the air; a greater number being an argument of greater purity in the air that he had examined. Because had the mixture suffered no diminution at all, it must have been perfectly phlogisticated.

Having no suspicion but that so able a philosopher, and one who had given such particular attention to this very business, must have been aware of any considerable fallacy to which it had been subject, I was not a little rejoiced at the discovery. For certainly the very different power of diminishing common air in different quantities of nitrous air is one of the greatest difficulties we have to contend with, in using it as a test of the purity of air. This satisfaction therefore I enjoyed a considerable time. But at length seeing some reason (I have now forgot what it was) to entertain a doubt
about

about it, I first submitted it to trial; when I presently found that, unlikely as it had appeared to me, both the Abbe Fontana, and our common friend, had certainly been guilty of some great oversight; the different state or quality of the nitrous air causing just the same uncertainty in his method of applying it, as in mine. I then, but not before, examined what my friend had advanced in praise of this method, when I also perceived a fallacy in his *reasoning* on the subject.

As it is always of importance to correct the mistakes of those who will not readily be thought liable to make mistakes, I shall first recite the *facts* that I have observed, and shall then argue from the nature of the thing; but in both I shall be as brief as possible.

I had a quantity of nitrous air, which, by long standing in water, was much impaired in its virtue, so that, with equal quantities of common air, the measures of the test were 1.42; when with fresh made nitrous air, they were 1.29. Then, to apply the Abbe Fontana's method, by which
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I was to expect that this difference would vanish, I mixed two measures of the old nitrous air with one of common air, and put them into a graduated tube, of which thirteen divisions were equal to one measure. I did the same with the fresh made nitrous air. The result was, that the common air and the old nitrous air occupied the space of thirty-two of these measures, while the mixture of common air and fresh made nitrous air occupied the space of thirty of them; so that, notwithstanding there was phlogiston enough in each of the quantities of nitrous air completely to saturate the common air mixed with it, the difference in the last dimensions was two divisions. And it is plain that there must be the same difference made by deducting these numbers from any equal numbers whatever; as from 39, which is three times thirteen. For one of them will be 7 and the other 9, which are the quantities that have disappeared in the process.

When I used four measures of each of the kinds of nitrous air, and one of the common air, there was still the same difference in the results. For since there could
be

be no farther diminution produced by these additions, it was only adding *equal things to unequal things*, which could never tend to bring them to an equality. I repeated the experiments many times, and always had the same result. I also repeated them, and let the mixtures remain all night before I applied the measure, and I likewise used dephlogisticated air as well as common air; but in all the trials the difference in the nitrous air made the same difference in the result, both in the Abbe's method of applying it, and in mine.

I shall now consider Dr. Ingenhoufz's *reasoning* on this subject; but, to be as short as possible, shall only recite his application of it to the example that he produces. "Let us suppose, says he, p. 175, "that after
 "three measures of strong nitrous air are let
 "up, and the saturation of the two measures
 "of the air under examination be completed,
 "the remaining column of air be found equi-
 "valent to three measures, and eight subdivi-
 "sions, or to 308 subdivisions, this number
 "subtracted from the 500 parts, or subdivi-
 "sions

“ fions of both airs employed, will give a re-
“ sult of 192, which is exactly the quantity
“ of both airs destroyed. Let us now again
“ suppose that the nitrous air employed was
“ so weak that, instead of three measures, six
“ were required before the saturation was
“ fully completed, and that thus the remain-
“ ing column of air in the great tube occu-
“ pies 608, instead of 308 subdivisions; we
“ shall find that the result will be just the
“ same, that is to say, that by subtracting
“ the 608 parts remaining from the 800
“ parts of both airs employed in the experi-
“ ment, there will be found exactly 192
“ subdivisions destroyed, and that thus, in
“ both cases, the accurate salubrity of the
“ air is ascertained. If such bad nitrous air
“ only was at hand as just now supposed, it
“ follows that a longer tube ought to have
“ been employed. This observation,” he adds,
“ which I owe entirely to the Abbe Fontana,
“ is in my opinion of the utmost consequence,
“ and throws a great deal of light upon the
“ nature of nitrous air, and upon its won-
“ derful property of destroying respirable air;
“ and

“ and it illustrates his ingenious theory of
“ this quality, which I hope the author will
“ soon publish; but which I have no right
“ either to claim, or to anticipate. In con-
“ sequence of this observation, we need not
“ be so anxious about the goodness of the
“ nitrous acid, nor about the strength of the
“ nitrous air.”

In answer to this, I would observe, that if three measures of the nitrous air, in consequence of which 500 subdivisions of the graduated tube were reduced to 308, were not sufficient to produce the greatest diminution, three equal quantities more, having, by the supposition, a greater effect upon the common air, must necessarily make a greater difference than between 608 and 800. For if the difference should be just 608, it would appear that the addition of more nitrous air had not been capable of making any change in it at all, contrary to supposition. Consequently, if the air to be examined had not been completely diminished before, so that the addition of these three measures would have produced a farther diminution, the
quantity

quantity remaining must have been less than 608 subdivisions of the tube.

This ingenious observation appearing to be ill founded, as well as contradicted by fact, it behoves us to be as attentive to the strength of the nitrous air that we make use of, as a test of the purity of the other air, as ever.

It is true that we are all naturally biaised in favour of our own peculiar methods; but, besides that of the Abbe Fontana is exceedingly operose and tedious, I do not see that it has, in any respect, the advantage of the very ready and simple method that I have hitherto made use of, and described in the introduction to my last volume. His making the inside of his measuring tube rough with emery, would be a great advantage, if, as he says, it prevented the water from adhering to it in drops, and thereby contracting the dimensions of the tube. But Dr. Falconer informs me, that he has prepared a tube in this manner, without finding any such desirable effect from it.

SECTION XX.

Of the production of nitrous air in which a candle will burn.

IN each of the four preceding volumes on the subject of air, I have treated of a species of nitrous air, procured sometimes by a direct process, but originally by a change in the constitution of common nitrous air, in which a candle burns either quite naturally, or with an enlarged flame, and sometimes also with a crackling noise, and vehemence, as if it was true dephlogisticated air, or a mixture of dephlogisticated and inflammable air. In every successive volume will be found several advances in the investigation of the nature and properties of this species of air, in consequence of my having given to it perhaps more attention than it will be found to deserve. But our attention is by no means always bestowed according to the real, or even the seeming importance of the objects of it, but to something in them that excites our curiosity.

curiosity. Now there is something so exceedingly remarkable in this species of air, especially its property of admitting a candle to burn in it, when it is still as fatal to animal life as any species of air whatever, that I have not been able to refrain from attending to it. This attention I shall still keep up, and though I am far from having satisfied myself with respect to it, my readers will find in this volume many new observations relating to it; and particularly a most easy method of producing it in the greatest quantity; whereas it was originally a tedious and uncertain process with me.

My conjectures concerning the constitution of this kind of air will be found to have been various. But having discovered it at one time in a continued process, of the solution of zine in spirit of nitre, always to come between the phlogisticated and dephlogisticated air, I supposed it to contain less phlogiston than phlogisticated air, and more than the dephlogisticated air; and therefore as it has some properties of a genuine dephlogisticated air, I am inclined, on the whole, to call

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it a *dephlogisticated nitrous air*. But if I adopt a language that seems to be authorized by my late experiments, I should rather say that this kind of air consists of a *dephlogisticated nitrous vapour*, diffused through a quantity either of nitrous or phlogisticated air; which nitrous vapour may be separated from the nitrous, or phlogisticated air, by water, it being absorbed by it, in the same manner as fixed air.

It will not appear surprising, however, that having originally produced this air in my attempts to throw more phlogiston into nitrous air than it naturally contained, and in the very same processes by which common air actually does receive phlogiston, as by means of iron filings and brimstone, liver of sulphur, &c. and it being also fatal to animal life, I was at first inclined to call it a *phlogisticated nitrous air*, or, on account of its remarkable property of admitting a candle to burn in it with an enlarged flame, an *inflammable nitrous air*.

Since a candle burns in this kind of air, and an animal dies in it, I think we are authorized

thorized to say, that it is of such a constitution, as to be capable of receiving phlogiston in a very great degree of heat, perhaps not short of a red heat, but not in that degree which is compatible with animal life. It is well known that many substances in chemistry can act upon one another, when they are hot, which do not at all affect one another when they are cold. This, therefore, may be the case with this kind of air, and substances containing phlogiston.

Again, it is evident, from this air being readily diminished by agitation in water, almost, if not quite as much as fixed air is, and then becoming phlogisticated air, that that part of it which is capable of combining with phlogiston, has a considerable affinity to water, and is thereby capable of being entirely separated from the rest of the air in which it is found. It is not, however, fixed air, both because it does not extinguish a candle, and because it doth not make lime water turbid. I should think myself happy, if I could procure a quantity of this air, or vapour, quite pure, unmixed with nitrous or phlogisticated

air; but though I have made several attempts for this purpose, they have hitherto been without success, except by first saturating water with the air, and then expelling it by heat.

I shall begin the recital of my late experiments on this difficult subject, with mentioning some unsuccessful attempts to procure it, and then the method I at length hit upon for procuring it in the greatest abundance.

Having first found this air by exposing nitrous air to a large surface of iron, then to liver of sulphur, to iron filings and brimstone, and by other processes which may be denominated *phlogistic*, because a quantity of phlogiston is supposed by chemists to be exhaled in them, I exposed nitrous air likewise to a large surface of *white paint*, by laying it on thin pieces of wood, and then introducing them into a jar of that air.

In about two months one third of the nitrous air, in these circumstances, disappeared, but I only found that its power of diminishing common air was much impaired. For with an equal quantity of common air, the measures of the test were 1.48; but a candle
would

would not burn in it. After one month more the measures of the test were 1.55 ; but still it extinguished a candle ; and seeing no farther diminution of this air, I put an end to the process. It is possible, however, that had the state of this air been accurately traced, that state of it in which a candle would burn in it might have been found. But the best chance of finding this air, in what may be called a *phlogistic process*, must be when the diminution is pretty quick. For, otherwise, as fast as this vapour which combines with phlogiston is formed, it is absorbed by the water.

For some time I thought I should have been able to make this peculiar species of nitrous air by mixing nitrous air ready formed, with some other kind of air ; and considering that iron exposed to nitrous air becomes a calx, and therefore had parted with its phlogiston, and possibly in the form of inflammable air, I mixed inflammable air with nitrous air in various proportions, but without effect. This mixture, I observed, burned in the neck of the phial, that is, by

the help of common air, with a green or yellow flame. But the appearance is very unlike that which is produced by this air, which admits a candle to burn in it, without any assistance from common air.

I then thought it possible that what I could not produce by an immediate mixture might be effected by the same mixture in length of time. Accordingly I mixed nitrous and inflammable air in various proportions, and kept the mixtures many months; but this also was without effect. In some of the cases I made use of inflammable air from the marshes, which burns with a lambent flame, but with this I succeeded no better.

All these mixtures were diminished as nitrous air alone would have been. It was, therefore, the same thing in effect as throwing a quantity of phlogisticated air into the mixture, which in some measure varied the appearance of the accension. But except this, which I do not think worth noticing, nothing remarkable occurred in consequence of keeping these mixtures so long. It is plain, indeed, that instead of getting an addition of
phlogisti-

phlogisticated air, or of phlogiston in any form, I wanted a substance with which phlogiston might combine, and therefore a thing of a quite opposite nature.

At length I succeeded in a method of producing this air, when I expected quite a different result. As iron will precipitate copper from a solution of it in spirit of nitre, and I had observed air to be generated in this process; I imagined that by putting iron into that solution of copper in spirit of nitre, I should get more nitrous air, and with less expence. But instead of this, I procured what I should much more have wished for, viz. this new species of nitrous air. But before I hit upon this, I succeeded in the method of changing fresh nitrous air into this species of it, in a remarkably short space of time.

Having at hand a phial filled with nails, which had often been employed in diminishing nitrous air, I filled it up with a diluted solution of copper in spirit of nitre, and left it all night. I then displaced the liquor by nitrous air, and in about two hours

the whole quantity was diminished one half, and a candle burned in the remainder with an enlarged flame.

In this experiment I collected no air from the iron itself; but now, with the view that I have mentioned above, I filled the phial containing the nails with the solution of copper in spirit of nitre, and inverting it the next morning I found the phial full of air, and it was not proper nitrous air, as I had expected, but that species of it in which a candle burns. In this case it burned quite naturally, and without any enlarged flame.

The quantity of this peculiar species of nitrous air that may be procured in this manner, and from the same materials, viz. without changing either the iron or the solution of copper, is astonishing. I made however, a pretty full trial of it, in a jar which I had thrust full of long pieces of iron wire, on purpose to expose as much of the surface of the iron as I possibly could, to any kind of air that I should afterwards put into it. This jar, which held about a pint and a half, I filled with the diluted solution
of

of copper, and inverted it in a basin of the same solution; when, at the first, the jar was quite filled with this air in a few hours.

However, having got as much of this air as I wanted, I observed that more air was still generated; and inverting the jar every day, after I had filled it with the same solution of copper that had been expelled by the generated air the preceding day, it never failed to produce a jar full of that air every day for at least a fortnight, besides what escaped towards the beginning of the process from the mouth of the jar, as soon as it was full, and which I never collected.

I had imagined that, in time, the quality of this air would change, but to the very last it yielded air of the same kind in which a candle burned, not only naturally, but in a very vivid manner. If, however, I suffered the air to continue long in the jar, I always found it to be phlogisticated air; which, indeed, is the state to which this species of air is always reduced by long exposure in the same circumstances in which it is generated; the water either absorbing the dephlogisticated

cated vapour, as it may be called, or this vapour getting saturated with phlogiston.

It was something remarkable, that a phial of nails which had often been used to diminish nitrous air, when filled with water only, yielded phlogisticated air. I at first filled this phial with inflammable air, and had observed a constant addition to it. It required, however, the warmth of a fire, when the phial was filled with water, to make it produce any considerable quantity of phlogisticated air. These nails had probably a quantity of nitrous acid mixed with their rust, which might continue to act upon them; and this air, in its nascent state, might be nitrous, but afterwards became phlogisticated, according to the usual course of this process.

SECTION XXI.

Of the constitution of dephlogisticated nitrous air.

ONE of the most uncertain circumstances attending this species of air which is the subject of the last and present section, is that sometimes it will diminish common air, almost as much as fresh made nitrous air, and sometimes it has no such power; and I have not yet been able to say before hand when it will be possessed of this power and when it will not. As this dephlogisticated vapour of nitre is a thing quite distinct from the air with which it may be mixed, this may be either proper nitrous air, or phlogisticated air, that is, in this case, phlogisticated air produced from nitrous air. For the progress of the production of this air seems to be as follows. Part of the nitrous acid is first loosed from that union with phlogiston which

which constitutes nitrous air, and then becomes what I call *dephlogisticated nitrous vapour*, diffused through the remaining nitrous air. In time the whole of the nitrous air is thus decomposed, so that no proper nitrous air remains in the mixture. In this state I imagine the air shews the strongest appearances of dephlogisticated air, by admitting a candle to burn in it with a vivid flame. But when the nitrous air has suffered that decomposition, which lets the dephlogisticated vapour of nitre escape, this vapour is either imbibed by water, or forms with the phlogiston another kind of union, viz. that which constitutes phlogisticated air; and when the whole of the nitrous air has undergone this change, having parted with all the phlogiston which it held *as nitrous air*, it is no longer capable of attracting common air. Lastly as the dephlogisticated vapour is either absorbed or saturated, the proportion of phlogisticated air is greater, till at length it consists of nothing else.

Whether this theory be supported by the following facts, in which this air sometimes did,

did, and sometimes did not affect common air, I leave to the judgment of my readers.

Dissolving zine in strong spirit of nitre, I got a considerable quantity of air in which a candle burned very vigorously, hardly to be distinguished from dephlogisticated air, and it had no effect on common air. But at other times I have got air from this solution that was pretty strongly nitrous.

Nitrous air exposed to iron from the 24th of June, and diminished to about one third, admitted a candle to burn in it with a flame somewhat larger than natural, very bright, and slightly blue all round. In this state, I doubt not, it would have diminished common air, but when it was examined again on the 17th of July, it was neither diminished by nitrous air, nor did it affect common air in the least.

Two quantities of nitrous air exposed to iron the 23d of July were diminished between one half and one third on the 31st of the same month, when they admitted a candle to burn in them with a very vivid flame, and did not diminish common air at all.

Lastly,

Lastly, a quantity of nitrous air, in contact with iron from the 2d of September was diminished one half; and on the 13th of the same month it affected common air very little, the measures of the test being 1.75. It burned with an enlarged flame.

In all these cases, in which the common air was not much, if at all affected by this air, a candle burned in it very vigorously; all the nitrous air being decomposed, and not much of the dephlogisticated vapour absorbed. In the remaining trials the common air was affected.

A quantity of this air produced by a solution of tin, and in which a candle burned with a blue flame surrounding it, diminished common air in part. This blue flame is an indication of a mixture of some proper nitrous air. For with that colour a candle is always extinguished in nitrous air.

A quantity of this air (I did not note how it was procured) in which a candle burned with a bright flame, surrounded with a thin blue one, diminished common air almost as much as fresh nitrous air. By
standing

standing all night in water, about one fourth of it was absorbed, but still a candle burned in it naturally, and it diminished common air not much less than before. In less than twelve hours afterwards, it extinguished a candle, and then did not affect common air near so much.

These facts are not so agreeable to the preceding theory as one might wish; yet this blue flame was an indication of nitrous air, and the quantity of air absorbed shews that there was much dephlogisticated vapour mixed with it, which supported the bright flame in the centre. The diminution in this case might be the effect of the absorption of the dephlogisticated vapour, of which this air contained so much. An attention to the redness of the mixture of it with common air might have helped to discriminate in this case. The next observation is similar to the above.

A quantity of this kind of air in which a candle burned with a vigorous white flame, surrounded by a thin blue one, diminished common air so much, that the measures of
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the test were 1.26. The next day this air, having stood exposed to water all night, the manner in which a candle burned in it was not sensibly different, though one third of the whole quantity had been absorbed. The measures of the test then were 1.24. The next day one half of what remained was absorbed, and a candle went out in the air. The measures of the test were 1.34. The day following the air was diminished one fifth more, and the measures of the test were nearly the same. This must have been pure nitrous air, mixed with very much dephlogisticated vapour.

Another quantity of this air, in which a candle burned with a strong white flame, in the centre of a light blue one, diminished common air so much, that the measures of the test were 1.28. The next day it was diminished about one sixth, and just extinguished a candle. Mixed with common air the measures of the test were 1.3. It appears from this experiment, that a very small quantity of this dephlogisticated vapour suffices

fices for the burning of a candle in the air with which it is mixed.

A quantity of this air that approached the nearest to the state of dephlogisticated air, with respect to a candle burning in it, was some that had been nitrous air, had been exposed to iron the first of July, and was examined on the 17th. In this case the flame was exceedingly bright, attended with a crackling noise, insomuch that, had I not known the manner of producing this air, I should have pronounced it to be proper dephlogisticated air, and yet it would certainly have been fatal to animal life.

Having tried the effect of the flame of a candle in this air, I had little doubt but that inflammable air might be ignited in it; nor was I disappointed. For when the two kinds of air were mixed, they exploded at once, and with considerable violence, almost equal to that of a similar mixture of inflammable and dephlogisticated air.

I then put a pot of iron filings and brimstone into a jar of this air, to try what alteration would be made in it by such a gradu-

al and equable supply of inflammable air, as that mixture would throw into it. After continuing in this state four or five days, I found the air, though confined by water, very little diminished. However, a candle then went out in it. It was farther diminished by agitation in water. But I always found this air capable of being considerably diminished by agitation in water, after it had been so far affected, by a previous standing, or agitation, in water, that a candle would just go out in it.

That it is a mixture of pure nitrous air that is the cause of this thin blue flame with which the central flame in these experiments is sometimes surrounded, is, I think, evident from the following experiment. Having a quantity of this air in which a candle burned with a strong and bright flame, I mixed nitrous air with it, and then the candle burnt in it with the usual enlarged flame, a bluish flame surrounding the former.

In order to determine as nearly as I could, how the property of admitting a candle to
burn

burn in this air, or of extinguishing it, corresponded to the quantity of its diminution, and also of that of the common air by means of it, I made the following experiments, which however, when compared with those which have been mentioned before in other views, only give a general idea of this progress; and great varieties, I doubt not, may be found in different quantities of this air, in all the respects abovementioned.

Having prepared a quantity of this air by keeping nitrous gas in contact with iron twelve days, I found that it bore agitation in water till near one half of it was absorbed, before it would extinguish a candle.

Having again put a quantity of nitrous air to iron the 21st of September, the next day its power of diminishing common air was not sensibly changed. How much it was diminished I did not note, but it was probably one tenth of the whole. On the 23^d the diminution was one twelfth of the remainder, and with an equal quantity of common air, the measures of the test were 1.25; when, with fresh nitrous air, they

were 1.3. In this case part of the diminution was probably an absorption of the dephlogisticated vapour. On the 25th the diminution was one sixth of the remainder, and the measures of the test were 1.37; by which it appears that this air had now lost some of its power of diminishing common air. On the 26th the diminution was again one sixth of the remainder, and the measures of the test were 1.42. On the 28th the diminution was one eighth of the remainder, and the measures 1.7. Dipping a flame of a candle into a portion of it, it burned with an enlarged flame. On 30th the diminution was again one eighth of the remainder, and the measures 1.8. Lastly, on the 2d of October, the diminution was one sixth of the remainder, and the measures about the same as before; so that now the diminution of common air by it was little or nothing. What now remained of the air was, I suppose, too small to dip a candle into it, but I take it for granted that it would have been extinguished.

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Among other methods in which I attempted to procure this dephlogisticated vapour quite pure, and freed from that part of the air with which it was mixed, and which either diminished common air, or extinguished a candle, that which I thought the most likely to succeed, was to saturate a quantity of water with this air, and then to expel it by means of heat. For then nothing could come out of the water but that part of the air which had been imbibed by it, of which the nitrous air, and especially the phlogisticated air, would be a very small proportion.

Accordingly I took distilled water, and made a quantity of it absorb as much as it would of this air; but when I expelled it again by heat, it admitted a candle to burn in it only just as it had done before. It was not converted into any better kind of air by the process; for it was not at all diminished by fresh nitrous air. I had entertained some small expectation that if I failed to get pure dephlogisticated vapour from this water, the air might come out changed in some other respect.

This experiment was made on the 4th of October. On the 13th of the same month I expelled air from what remained of that water, after it had been kept in a phial with a ground stopper, half filled with common air; when the air expelled from it was found to be much less in quantity than before, and it was diminished by nitrous air about as much as common air. By this it appears that the quality of it was considerably altered by its *continuance* in water; and also, probably, by means of the air incumbent upon the water; but in what manner this was effected, it will require more experiments to explain.

On the 13th of October I repeated this experiment, by agitating a quantity of this nitrous air in a state in which it did not affect common air at all, in distilled rain water; and expelling the air *immediately afterwards*, a candle burnt in it with a vivid flame, as before; and after the water had absorbed what it would of the air, a candle went out in the remainder, so that it was, in all respects, the same thing when it came out of the water, that it had been before it went
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into it. Perhaps, however, a less degree of heat might have expelled one part of this air, without the other. In proper circumstances that is, when confined so as not to be exposed, through the medium of water, to the common atmosphere, this kind of air remains unchanged both in quantity and quality, as much as any other species of air whatever. Having kept different quantities of it in a great variety of circumstances, from different periods, to the 24th of July, when I was obliged to put an end to the process, I took the following notes respecting this subject.

Two quantities of this air, which had been kept many months in phials with ground stoppers, admitted a candle to burn in them with a bright enlarged flame. One of these portions of air, agitated in water, was presently reduced to very near one half its bulk; and then, with an equal quantity of common air, the measures of the test were 1.66. The other extinguished a candle when, without agitation, it was diminished about one tenth only.

Another quantity of this air, exposed to an equal bulk of water, upon quicksilver, and frequently agitated, but which had not been sensibly absorbed by the water, admitted a candle to burn in it quite naturally. Also another quantity, which had been procured from iron by means of the diluted solution of copper in spirit of nitre, and in which a candle burned naturally the third of October 1779, and which had been confined by quicksilver, appeared to be, in all respects, unchanged at this time.

In vol. ii, p. 130, I have given an account of a mixed kind of air, from spirit of nitre, and oil of turpentine, to which, when a quantity of alkaline air was introduced, a white cloud was made, and part of the air disappeared. In this air a candle burned both before and after that process. This air, no doubt, contained a mixture of dephlogisticated nitrous air; but the white cloud must have been owing to a mixture of fixed air in it. For repeating the experiment with dephlogisticated nitrous air, procured from iron and a solution of copper in the nitrous acid,

acid, which contained no fixed air, I did not perceive the least cloud when alkaline air was admitted to it, nor the smallest diminution in the quantity of the whole, while they were confined by quick-silver. When water was admitted to them, it absorbed the alkaline air, and a candle burned in the remainder exactly as before.

This proves that the acid in dephlogisticated nitrous air is intimately combined with some other substance. The same appears, perhaps, still more clearly, by means of the juice of turnsole. For admitting a quantity of this air to water tinged blue with the juice of turnsole, part of it was absorbed, in one case about one half of the whole, without making any change in the blue colour.

SECTION XXII.

Of the production of inflammable from alkaline air; by the electric spark.

THERE are few experiments the *rationale* of which I less pretend to understand, than the production of genuine and permanent inflammable air from alkaline air, by means of the electric spark; the alkaline air being wholly imbibed by water, and the inflammable air produced in it, if not from it, not at all.

One query on this subject is, whence comes the phlogiston, which is certainly a principal ingredient in the constitution of inflammable air. Alkaline air, indeed, contains phlogiston, because, in the manner in which I have generally produced it, it is itself partially inflammable; but it is not nearly so much so as the inflammable air, which is produced by means of it. Besides, it will appear by the following experiments, that the quantity of the inflammable air far exceeds that
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of the alkaline. If I might indulge a conjecture on the subject, it should be, that the phlogiston of this inflammable air is supplied by the electric matter, and that something which serves for a *basis*, as it may be called, is supplied by the alkaline air. For though inflammable air approaches the nearest to the state of pure phlogiston of any substance that is known to us, it is, I doubt not, composed of phlogiston and *something else*; and this experiment may perhaps shew, that this basis, which has hitherto been unknown to us, is of an alkaline nature. For taking the electric spark in any species of acid air, has no such effect. From this fact also we may perhaps be led to imagine, that phlogiston has a nearer relation to an acid nature than to an alkaline one; so that it readily combines with the latter, as with the earth of metals, &c. rather than the former.

At the time of my former publication on this subject, I had only ascertained, though very decisively, the general fact, viz. the undoubted production of a true inflammable air, exactly like that which is produced from iron

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or zinc by solution in oil of vitriol or spirit of salt, in consequence of taking the electric spark in alkaline air. But I have since ascertained the *quantity* of inflammable air that may be produced from any given quantity of alkaline air. And this production having its limits, certainly shews that the alkaline air supplies some essential part in the constitution of it, and that the whole of the inflammable air is not derived from the electric matter, the alkaline air serving only as a medium, as it might otherwise have been imagined, in which the process takes place.

To take my measures with more accuracy, I, at this time, confined the alkaline air in a glass tube, of the same dimensions throughout; and having it confined, as it necessarily must be, by quick-silver, I carefully marked the space which it occupied in the tube. I then took the electric spark, or explosion, which ever of them happened to be the most convenient, till I perceived that no more addition was made to the quantity of air; and then measuring the space which it occupied, I found that the whole was, as
nearly

nearly as possible, three times as much as that which the alkaline air alone had occupied. When I examined this air, I found it to have an inflammability of the strongest kind, firing with explosions, and in no respect to be distinguished from that which is extracted from metals by acids. Also the electric spark taken in this air was always red, though, as is also the case with other inflammable air, it was white in the centre of any considerable explosion taken in it.

If the theory suggested above be the true one, this experiment may prove that the proper inflammable principle, supplied by the electric matter, constitutes two thirds of the bulk of inflammable air, and the alkaline principle only the other third. We are not authorized however, to infer from it any thing at all concerning the separate *weights* of the alkaline and inflammable principle. It is even possible that the phlogiston may have no weight at all, though it enables the alkaline base, on which it has seized, to occupy so much more space than it did before.

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After this experiment it still remained a doubt whether, when the process was completed, there did not remain, at least, some portion of alkaline air not affected by it, and capable of being absorbed by water afterwards. To determine this, and likewise to repeat so important an experiment upon a larger scale, I began with one third of an ounce measure of alkaline air, and I took the electric spark in it till I had got a complete ounce measure of air. Then admitting a little water to it, I observed with the greatest attention, but could not perceive that any part of the air was absorbed by it. However, when I had made this air explode, by means of the flame of a candle, and immediately after applied my nostrils to the mouth of the vessel in which it had been contained, I perceived a very evident alkaline smell; so that the whole of the volatile alkali had not been completely incorporated with this air, though it was so much so, as not to be seized by the *water*. And to give it a fairer trial, this water had been confined along with the air, upon the
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quick-silver, and had been even frequently agitated with it, during two whole days; and though it was but a very small quantity, it had no perceivable alkaline smell afterwards.

If the theory concerning the constitution of inflammable air, advanced by way of conjecture above, be admitted, it will favour the supposition in my first volume, p. 106, that part of the calx of the metal, from which the inflammable air was produced, enters into the constitution of the air, and is the proper basis of it; the calces having some properties of an alkaline substance. It may also prove the convertibility of one alkaline substance into another, or at least such a change as makes them to become the same thing, that is, to have all the same known properties in the constitution of air; in the same manner as there is something common to the vitriolic and nitrous acids, which cannot be distinguished when they enter into the constitution of dephlogisticated air. Since inflammable air, when procured from any of the metals, by any of the acids, or from
alkaline

alkaline air by the electric spark, exhibits, in all respects, the very same properties, it can hardly be imagined that its composition is really, or, at least, materially, different. But this cannot be ascertained completely till we are able to decompose any kind of air, and collect again the different elements out of which we had previously formed it; and this is a problem, of the solution of which, after what has been already done on this subject, we ought not to despair.

Do not these experiments lead us to conjecture, that the inflammability of alkaline air, does not arise from the *alkaline principle* itself, or from any thing necessarily connected with it, but from phlogiston accidentally adhering to the materials from which it was expelled, readily forming with this air, as a basis, a species of inflammable air; and will not phlogiston set loose in other processes have the same effect in alkaline air?

SECTION XXIII.

Experiments proving the great volatility of quick-silver.

THAT Mercury is volatile, even in the temperature of the atmosphere, when its surface is exposed to a *vacuum*, has been long evident from observations on the barometer, in some of which, exposed in the sun, a perfect distillation is perpetually going on; the invisible mercurial vapour always rising on the warmer side of the tube, and then forming into globules, and running down the opposite side, in the form of dense fluid mercury. But the experiments I have lately made seem to shew that this heavy substance is not less volatile when confined by vitriolic acid air, though pressed with the weight of the atmosphere, and that it is in some measure volatile, even when exposed to common air.

Presently after the discovery of vitriolic acid air, I observed that when the electric explo-

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sion was taken in it, which was done by confining it with quick-silver, in a glass syphon, so that the electric matter was made to pass from the mercury in one leg of the syphon to the mercury in the other, the tube was presently covered with a black incrustation, and the longer the explosions were continued, the thicker this incrustation grew. I had not at that time, however, any suspicion that this black matter came from the quick-silver, but imagined that it was altogether formed from the vitriolic acid air. This I was then led to conclude from there being no such appearance when the electric spark was taken in marine acid air, though confined by mercury, in the very same manner.

Afterwards, observing the same black matter, though not procured with the same ease, or in so great a quantity, when the explosion was taken over mercury in common air, I could not help suspecting that this black matter came from the mercury; and this suspicion was confirmed by applying heat to it; for it was thereby converted into white fluid mercury. I thought, however, that it
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was produced by the electric explosion volatilizing the mercury, in consequence of falling directly upon it. For though the heat occasioned by such an explosion be confined to a small space, it is exceedingly intense.

That the explosion might not affect the fluid mercury, I next took it between two iron wires, half an inch above the surface of the mercury, in the vitriolic acid air confined by it, and still had the black matter; which made it evident that the electric explosion did not produce the evaporation of the mercury, but found the mercurial vapour dispersed in the air. I also made the same experiment, and with a similar result, in common air. But in this case I could not produce the black matter, at least in any sensible quantity, at any considerable distance above the surface of the mercury; and in no respect were the appearances so striking, as when the explosions were taken in vitriolic acid air.

I took the electric explosion between iron wires at the distance of several inches above the surface of the mercury in this kind of air, and the blackness within the tube was

produced just as much as it had been when the explosion was taken immediately upon the quick-silver itself; and on applying heat to the black matter formed in these circumstances, it presently became running mercury as before.

Having taken the electric explosion at various distances above the surface of the mercury by which the vitriolic acid air was confined, and always with the same success; I at last took it at the greatest distance that any glass tube I had by me would admit, which was about three feet above the surface of the mercury. But even in this case the black matter was, to all appearance, produced quite as readily, as when the explosions had been taken ever so near to the surface of the mercury; so that the mercurial vapour had completely pervaded this whole space of vitriolic acid air, and in a very short time; for I took the explosions presently after I had prepared the tube for the experiment.

But to be quite sure that this black matter did not proceed from the vitriolic acid air, I contrived to take the electric explosion in
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it when it was not confined by mercury. To do this, I completely saturated a quantity of water with this kind of air, confined in a glass tube, in the top of which I had cemented a piece of iron wire, which came within a proper distance of the extremity of another piece of wire, which reached to the bottom of the tube. The impregnated water was confined by mercury in the tube, and in the basin.

In these circumstances, a small degree of heat made this water give out its air; so that all the upper part of the tube was filled with it, resting on the water only. Between these two wires I took large electric explosions a considerable time, but no black matter was produced. It is evident, therefore, that this black matter consists of mercury *superphlogisticated*; the phlogiston coming from the electric matter when the explosions are taken in common air, but chiefly from the vitriolic acid air which abounds with it, when they are taken in that air; and this accounts for the appearances being so much

more remarkable in this kind of air than in common air.

But though, in my experiments on mercury, an account of which is given in my last volume, mercury superphlogisticated by agitation in water, and assuming the form of a black powder, becomes white running mercury the moment that it becomes dry, this was not the case with the black matter in this process. However, when I moistened a little of it, and dried it again, I thought that part of its blackness disappeared, though not very sensibly.

Ethiops mineral is a composition of mercury and brimstone, and therefore resembles the black matter produced by these electric explosions in vitriolic acid air, and the vapour of mercury; the vitriolic acid air alone, as I have shewn, becoming sulphur in certain circumstances. I thought, therefore, that this black matter might be a real ethiops; but when I put a little of it upon hot iron, I did not perceive any blue flame to arise from it. If, therefore, this black mat-
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ter be an ethiops mineral, the proportion of sulphur in it must be exceedingly small.

It still remained to be determined, whether this diffusion of mercurial vapour through the vitriolic acid air was occasioned by a proper *evaporation*, that is, by the repulsion of its particles, whereby it is made to assume an elastic form, and in that state to mix with the air; or whether there be a *chemical union* formed by the mercury and this kind of air, and it therefore becomes *incorporated* with it. The following experiment seems to decide in favour of a proper evaporation.

I put a small globule of mercury into a narrow glass tube, communicating with the inside of the phial in which the oil of vitriol and copper for the production of vitriolic acid air were contained. But though I heated these materials, and continued the production of vitriolic acid air in these circumstances a long time, so that the globule of mercury was always kept exposed to a torrent of this kind of air, newly generated, I

saw no prospect of its being at all diminished by it. I therefore conclude that vitriolic acid air does not properly take up, so as to combine with, the mercury. However, it must be acknowledged to be difficult to account for the quantity of mercury contained in this black matter in whatever manner it becomes diffused through the air, considering that this globule of mercury was not sensibly diminished. This, however, might possibly be owing to its being continually surrounded with a little moisture, from which I could not keep it free; owing perhaps to the oil of vitriol not being sufficiently concentrated, so that the watery part was thrown off by the heat.

SECTION XXIV.

Of the presence of the nitrous acid in the calces of metals.

ALL the nitrous metallic salts have been distinguished by their property of *deliquescence*; but in my experiments with a long continued sand heat, of which an account is given in my last publication, I produced two of these saline substances, which did not deliquesce at all. They were produced from diluted solutions of copper and of mercury in the nitrous acid. The crystallizations were formed during the action of heat, in glass vessels hermetically sealed; and they were dissolved again in the same menstruum, when it was cold. But when the vessels were broken, and the saline substances were exposed to the air, they attracted no humidity at all; and yet they were not mere calces, because they were exceedingly caustic, and had a most disagreeable taste. I have
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since produced a saline substance of this kind from *iron* in a much less space of time, and the examination of it may throw some light on the constitution of the others.

A diluted solution of iron in nitrous acid, being only exposed one day to a pretty strong sand heat, in a glass tube hermetically sealed, all the iron seemed to be precipitated, and the liquor was left nearly colourless. This liquor afterwards dissolved iron as before, so that the action of heat in these circumstances, viz. under a strong pressure, and when nothing can escape into the open air, seems to oblige the acid to quit its hold of the metal, in a great measure. It is indeed the property of nitrous solutions of iron, that they will always make a deposit, and then dissolve more iron, I believe without limits; but then the colour of the acid always continues red.

By this process, therefore, this remarkable property of the nitrous acid seems to be increased with respect to iron, and may perhaps be extended to the other metals. I have not indeed as yet extended this operation to
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the other metals, and therefore it must at present be considered as a mere conjecture. But as I am now situated in a country where long continued fires will be less expensive, I hope that, if all be well, my readers may hear from me again on this subject, and others of the same kind.

The iron precipitate was by no means a mere calx; for it had a very acrid taste.

With copper a considerable time seems to be absolutely requisite to produce these non-deliquescent crystals, as appears from the following experiment which was likewise attended with some other circumstances, that I am not able to explain. A quantity of a weak but saturated solution of copper in spirit of nitre, which had been exposed to a sand heat about a week, and in which some crystals were formed, had many more crystals formed in it; so as to become like a thin paste, presently after it was poured out of the tube. But when the whole mass was dissolved by heat, in the open air, and then dried, it became perfectly deliquescent; unlike that which had crystalized before in a longer continued heat.

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That excellent philosopher, and most amiable man, Mr. Fabroni, who is as communicative as he is intelligent, informed me that the calx of tin would dephlogisticate spirit of nitre, and leave it colourless. This I found to be true; but then I found that, together with its colour, the acid lost almost all its strength. And trying other metals, I presently found that the earths of all of them have a remarkably strong affinity with the nitrous acid, and firmly uniting with it and a little water with which it is combined, make together a perfectly dry substance, quite unlike what it was before; the water being no more apparent, than it is in dry flaked lime. But heat will discover the water in both the cases.

Of this kind of calx, which I think we may properly term *nitrated* is the white minium, which I had before procured by saturating red lead with nitrous vapour; the phenomena of which, as I have found them to extend to other metals, I now understand better than I did before. I thought it something extraordinary, that a red substance,

stance, like minium, should, by the addition of a red and highly phlogisticated vapour, become a white substance. But I find that all the metallic calces on which I have tried the experiment do also become white, when they are, in like manner, saturated with spirit of nitre; and that this may be effected by a much easier process than I thought of before.

The production of the red vapour of spirit of nitre by means of bismuth, and other metals of which it makes a rapid solution, will be a difficult and unpleasant process to most persons; and those who are most expert in experiments of this kind, will be obliged to make several trials before they succeed to their wish, in some of the experiments that I have reported. But I now make all these nitrated calces by means of the simple distillation of weak saturated solutions of any of the metals.

In this process the greatest part of the water is evaporated, and the acid, together with a small portion of the water, firmly unites with the calx of the metal, and, together

gether with all the phlogiston that the metal contained, is deposited in the form of a white powder, which is incapable of being re-dissolved, either in the same menstruum, or in water. This deposit of white matter is made during the whole course of the distillation, in which nothing comes over but water; and the whole of the metallic calx becomes a white nitrated powder, as described above. This, at least, is the case with copper; and though I did not make the experiment in the same manner with *tin*, the phenomena, in a similar process, were the very same. There will probably, however, be considerable differences when the process is extended to other metals; and if I go through with these experiments, I shall not fail to report them.

In distilling a quantity of that solution of copper, which remains after making nitrous air (of which about one twentieth part is strong spirit of nitre, and the rest water) but fully saturated, there came over a transparent liquor, which had little or no taste; and from the very beginning of the process, I observed a constant deposition of white matter,

ter, which kept increasing, till the greatest part of the fluid was expelled. This matter I collected and dried, when it remained a perfectly white powder, but was easily discovered to contain much concentrated nitrous acid. For when I exposed it to heat in a glass tube, it emitted a copious red vapour, together with a good deal of liquid, and exhibited all the phenomena that I had before discovered in white nitrated minium, and in the calx of tin, on which I had distilled spirit of nitre. For, beginning with the idea that Mr. Fabroni had given me, I first put the spirit of nitre upon the calx of tin, and afterwards upon the tin itself; but I had the same produce of white nitrated powder at the last. That calx of tin which was yellow was made perfectly white by the distillation of spirit of nitre upon it.

The experiment of lead I made in a different manner, as follows. I dissolved seven dwts. of lead, in spirit of nitre mixed with about an equal quantity of water, when some air was produced, but not much. The bulk of what remained was a white powdery substance,

stance, covered with a small quantity of liquid, at first green, but afterwards transparent. Transferring the whole into a cup, and rinsing the phial in which the solution had been made, I observed that the white substance, which was *nitre of lead*, was immediately dissolved by the water. Placing the cup in which the whole was contained near the fire, it became almost all liquid, and transparent, the menstruum being enabled by heat to hold in solution a much greater quantity of this nitre of lead.

When by this exposure to heat, all the moisture was evaporated, and it was made perfectly dry, it weighed eight dwts. so that there was an addition of one dwt. from the acid and the water that were now latent in this calx. In this manner, however, it was brought to the same state with the nitrated calces of copper and tin abovementioned. For when heat was applied to this white substance, a red vapour was expelled from it, but seemingly combined with more water.

Having, in this, or some other similar manner, procured white nitrated calces of
lead,

lead, zinc, copper, and tin, I inclosed a little of each in separate glass tubes; and then, with a blow pipe, applied to them the flame of a candle; when they all emitted red vapour, and as soon as the tubes were quite filled with it, I closed them all hermetically, before any air could be admitted.

Letting these tubes remain some days, I observed that the red vapour was reabsorbed by all the calces, but less slowly by the calx of lead than by those of tin or copper, and most quickly by that of zinc. N. B. I found it exceedingly difficult to expel all the moisture from the solution of zinc in spirit of nitre; but when this was effected, I had a true nitrated calx of this metal, as well as of the rest.

This experiment discovered to me a mistake I was under with respect to my last directions for filling of glass tubes with the red vapour of spirit of nitre. Instead of doing it directly, from the solution of bismuth, which is a difficult and disagreeable operation, I advised to procure, in the first place, a quantity of what I now call *nitrated calx of lead*;

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and putting some of it into a glass tube, closed at one end, to heat it till the whole tube be filled with the red vapour, and then immediately to seal it hermetically. This direction will still be right, provided that presently afterwards that end of the tube which contains the nitrated calx of lead be taken off, by melting the tube just beyond it, which indeed I then also advised to do, though I had not then discovered the principal reason for it. For if the white calx from which the red vapour was expelled be suffered to remain long in the tube, it will reimbibe the whole of it. But then the vapour may be expelled again by heat, and will continue to fill the tube a considerable time.

When I first produced the nitrated calx of lead, it was by means of a rapid solution of pieces of bismuth; and the vapour was conveyed immediately from the vessel in which the solution was made, through a bent tube connected with it, into the other vessel, in which I had placed the red lead. But this vapour, as I then observed, was by no means dry; and small drops of a very blue spirit of
nitre.

nitre were frequently falling from the end of the tube out of which the vapour issued. This degree of moisture I find greatly facilitates the absorption of the vapour.

Willing to try the effect of a perfectly *dry* nitrous vapour, I made the solution with the apparatus described vol. iii. fig. 3, interposing one of the inverted phials between the two vessels that I made use of before; and at first I concluded that this dry vapour would not be imbibed by the minium at all. But I found, after some days, during which it had been confined in a phial with a ground stopper, together with some minium, that it was completely absorbed, and the red lead became white as before. I propose to repeat with this *dry* vapour most of the experiments which I formerly made with the *moist* vapour.

SECTION XXV.

Of the mixture of nitrous and vitriolic acid.

I HAD before observed some pretty remarkable phænomena that attend the mixture of the nitrous and vitriolic acid, particularly a turbid appearance, and a white deposit, though both the acids were perfectly transparent. Thinking that this might possibly arise from some extraneous earthy matter, in the oil of vitriol, I repeated the experiment with a quantity which had been first distilled, and then concentrated, and with a nitrous acid the purest and the palest that I could make. But this mixture was attended with the same phænomena as before, namely with heat, and a turbid white deposit.

I collected a quantity of this white deposit, and found that it was compleatly dissolved in spirit of salt, and gave it a yellow colour; so that it seems to be the same thing that is deposited when the vitriolic acid is distilled

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to dryness, and therefore to be something contained in this acid, and probably essential to it. This earthy matter seems to deserve a more particular consideration.

One of the most extraordinary circumstances that I have hitherto observed relating to this mixture, is the extreme volatility that it seems to give to the nitrous acid, so that, as far as I can yet perceive, the whole of it makes its escape from the mixture. This observation was at first quite casual. For having left the mixture, consisting of equal quantities of the strongest kinds of each of these acids, in a phial with a ground stopper, about four months, in which I had been absent from home, I found, at my return, the stopper driven out, and nothing in the phial besides the vitriolic acid, and, as far as I could judge, quite pure. For when I dissolved iron in it, nothing but inflammable air was yielded, even from the beginning of the process, and no mixture of nitrous air at all. Also the vitriolic acid was much weaker than it had been; so that it had been diluted

afterwards by imbibing water from the atmosphere.

I had the same result from another mixture of equal quantities of the two acids, which had stood in the phial without a stopper from the 6th of June to the 23d of July; and the quantity was diminished only one-fourth of the whole.

I also exposed, during the same time, to the open air, some of the crystals which I had observed to be formed by the impregnation of the vitriolic acid with the nitrous acid vapour. The consequence was, that the crystals gradually dissolved, and the quantity of liquid increased, till it exceeded twice the bulk of the crystals. When I dissolved iron in this liquid, I got nothing but inflammable air. When the very first produce of it was mixed with common air, there was no sensible diminution of it, so that there seemed to be no nitrous air produced.

Another method of separating the nitrous from the vitriolic acid, and in much less time than the above, was by exposing the mixture

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to nitrous air. This I have observed phlogisticates nitrous acid, and renders it extremely volatile; so that a very great proportion of it escapes. And when it is mixed with the vitriolic acid, and exposed in the same manner, the whole of it seems to escape.

Having introduced a phial of this mixture into a jar of nitrous air, in the same manner as I had before treated the nitrous acid itself, I observed that it absorbed the nitrous air as fast as the pure nitrous acid alone had done. Immediately after the process was commenced, it was covered with a dense red vapour, and gradually assumed a light orange colour throughout, beginning at the top. When the whole of it had acquired this colour, I withdrew it, and exposed it twenty-four hours to the open air; after which the top was become of a light blue, and the bottom of a yellowish colour. I then put it into another jar of nitrous air, and suffered it to remain there a fortnight, during which time I was absent on a journey.

At my return I found the mixture quite colourless, though it had absorbed little more.

of the nitrous air. I then dissolved iron in it, and it yielded nothing but inflammable air, of the strongest kind, without the least mixture of nitrous air; the very first produce of it not in the least affecting common air. The water in the jar in which this process was made yielding air copiously, I collected a quantity of it, and found it to be strong nitrous air. It had been produced by the impregnation of the water with nitrous vapour.

In order to discover in what *time* this effect might be produced, I repeated the experiment, and found that after being exposed four days to nitrous air, it became colourless, and the air produced by it from iron was all inflammable air.

If the vitriolic and the marine acids be mixed, much, if not all, of the marine acid is presently expelled, in the form of marine acid air. I was willing to try what would be the effect of adding this acid to the mixture of the two others abovementioned; and I observed that, when I had poured a small quantity of a perfectly colourless marine acid, very gently upon the other, presently after
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they had been mixed, and while they were yet turbid, the marine acid remained transparent upon them both; but the place of contact presently became of a beautiful yellow or orange colour, very small bubbles of air rising now and then from it.

The next morning the whole mixture was of a beautiful orange colour. When it was agitated, it frothed very much, and the air or vapour, escaped very rapidly, making, as it were, small explosions; but after every agitation the mixture seemed to be more viscid, the air escaping with more difficulty. After the agitation, it remained of a paler colour than before. Probably the marine acid air had been, in some measure, thrown out; and the next day it was perfectly colourless, like water.

Bits of paper and bits of wood were not sensibly affected by the mixture of nitrous and vitriolic acids, and they did not give it any colour; but a fly gave another quantity of it a brownish tinge, though not very soon. The next day that in which the vegetable matter had been immersed was of a light blue,

blue, and that into which the fly had been put was still of an orange colour, and rather deeper than before. Three weeks after this, both these mixtures having been a long time quite colourless, I dissolved iron in them; and they both yielded inflammable air only; so that, if this be any proof of the absence of the nitrous acid, this acid had now entirely left the mixture.

It may be doubted, however, whether this be a sufficiently accurate test of the absence of the nitrous acid; though, when I formerly mixed these two acids, in an experiment of which an account is given vol. iii. p. 171, I found that iron dissolved in it yielded first nitrous, and then inflammable air, the former, no doubt, from the nitrous acid, and the latter from the vitriolic. I also found, in the course of these very experiments, that when I had kept a quantity of this mixture in a phial, with a ground stopper, from the first of June to the 23d of July, the first part of the produce of air by it, from iron, diminished common air very much, and there was the appearance of something nitrous even

in the last produce. For when it was exploded, there was a tinge of yellow or green in the flame. There was nothing red in the solution afterwards.

I shall endeavour to ascertain by other tests, whether there be any nitrous acid remaining in these mixtures.

SECTION XXVI.

Of the marine acid, and marine acid air.

SEVERAL years ago Mr. Woulfe informed me, that he thought that, by operating in my way, I should be likely to find something remarkable in the solution of *manganese* in spirit of salt; but, in a very friendly manner, he, at the same time, cautioned me with respect of the vapours that would issue from it, as, from his own experience, he apprehended it was of a very dangerous nature. He was also so obliging as

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to furnish me with a quantity of manganese for the purpose. I cannot say that it was the apprehension of danger, but rather having other things in view, that prevented my giving much attention to the subject at that time; and I should probably have deferred it still longer, had not Mr. Fabroni informed me of the dephlogisticating power of manganese with respect of spirit of salt, discovered by Mr. Bergman.

This information suggested a wish to procure a quantity of a perfectly dephlogisticated marine acid, in order to satisfy myself whether it would then yield any acid air, as it does in its common state, that is, when phlogisticated; suspecting that it would not, as I have always imagined that a certain portion of phlogiston is necessary to all substances, and especially acids, assuming the form of air.

The experiments that I have made upon this subject give much weight to this opinion, and at the same time throw great light on the general doctrine of these kinds of air. For it appears that the marine acid, when it
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is deprived of its phlogiston, is brought into a state very nearly resembling the nitrous acid; being then incapable of being exhibited in the form of air, that is, of air capable of being confined by quicksilver. For the moment that the vapour, which then issues from it, is admitted to quicksilver, it unites with it, and forms a white powdery substance, in the same manner as the nitrous acid vapour does; and when I resume these experiments, I shall probably find that, with oily and other substances, this dephlogisticated marine acid vapour will form compounds equally similar to those formed with them by the nitrous acid vapour. This is a new field that is yet before me.

From this analogy it is evident, that nothing is wanting to the nitrous acid vapour, to its assuming the form of *air*, but a sufficient quantity of phlogiston; and when it has got this phlogiston, it is *nitrous air*. This, therefore, is probably the nearest approach that we shall ever make towards bringing the nitrous acid into the form of air; and it is probably the combination of so much phlogiston with
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this acid, in the composition of nitrous air, that makes it not so readily absorbed by water, as the marine acid air, or vitriolic acid air; both which seem to be compounds exactly similar to that of nitrous air. I shall relate the experiments which led to these ideas in the order in which I made them.

I began with putting spirit of salt upon manganese, and then distilling it, as Mr. Fabroni had directed me; when the first observation that struck me, was a peculiar smell, exactly resembling that which is procured by dissolving *red lead* in the same acid. I then put a quantity of this distilled acid into a phial with a ground stopper, and a tube connected with it, and proceeded as I should have done to expel air from any other substance, with the flame of a candle, receiving the produce in quick-silver. On the application of heat, in these circumstances, it was easy to perceive that air, or vapour, was expelled; but it was instantly seized by the quick-silver, and formed a black crust.

Examining the air that was lodged at the top of the phial, and consequently had been
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mixed with this acid vapour, I found it very little, if at all, injured. This was owing to there being little or no phlogiston combined with the vapour, or separable from it.

I then fully impregnated a quantity of spirit of salt with manganese, by confining them together in the same phial; and I afterwards endeavoured to expel air from the acid thus altered. But still the vapour that came over immediately united with the quicksilver, and made a kind of amalgam with it, which, when dry, was a whitish or grey powder. The common air within the phial was not injured in this case, any more than in the former.

The above-mentioned powdery substance, being exposed to the heat of a candle on a piece of thin glass, evaporated in white fumes, but left behind it a small quantity of reddish matter, not very unlike red precipitate; which is another resemblance between the marine acid thus altered and spirit of nitre. After exposing this red matter for some time to a moderate heat, it became white, and sublimed without any sensible change.

change. When it was exposed to the focus of a burning lens, upon quicksilver, it yielded no sensible quantity of air. I had imagined that, at least during the presence of heat, the acid, which was latent in this white substance, might have assumed the form of air, but I was disappointed in that expectation.

The marine acid impregnated with manganese having the very same *smell* with this acid impregnated with red lead, I was led to repeat the preceding experiments with this substance also, and I had the same general results. For the vapour emitted by it instantly united with quicksilver, and formed with it a white powdery substance, of which, with a proper apparatus, I collected a considerable quantity; and I intend to subject it to various trials, of which I may possibly give some account hereafter.

A few miscellaneous observations relating to the *marine acid* shall conclude this section.

The black flakes, which remain after the solution of silver in spirit of nitre, are dissolved

olved by spirit of salt, and impart to it a yellow colour.

A solution of iron in strong spirit of salt was colourless; but being exposed to the air it became brown, beginning from the top; owing, I suppose, to the precipitation of the calx of the iron, as the acid and phlogiston escaped.

I had before observed that the vapour of marine acid does not injure air. I had the fairest opportunity of trying this; when I had exposed a quantity of this acid, in a glass tube hermetically sealed, during several months, to a sand heat. For the air within this tube, being examined several months after it had been exposed to that heat, was found to be not at all injured. The air which had been confined along with vinegar in the same manner, and the same space of time, was so far injured, that, with an equal quantity of nitrous air, the measures of the test were 1.44.

As a mixture of the nitrous and marine acid makes *aqua regia*, which dissolves gold, I had thought it might be possible, that

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common spirit of salt, after dissolving some of the nitrated calces above-mentioned, might have the same property ; but it had not. It is now pretty well confirmed, that it is the marine acid alone, in the composition of aqua regia, that dissolves the gold ; this acid being dephlogisticated by the spirit of nitre, which has a stronger affinity with phlogiston than the marine acid has.

SECTION XXVII.

*An investigation of the LATERAL EXPLOSION,
and of the electricity communicated to the
electric circuit in a discharge, from the
Philosophical Transactions, vol. 60, p. 192.*

SEVERAL years before I made any experiments in electricity except with a view to amuse myself and my friends, I had observed that, in discharging jars, particularly such as were filled with water, without any coating on the outside, I felt a slight shock ; though it was plain that the hand in which
I held

I held the discharging rod made no part of the circuit.

Mr. Wilson also, in his first experiments on the Leyden phial, observed that bodies placed without the electric circuit would be affected with the shock, if they were only in contact with any part of it, or very near to it. Analogous to this was his observation, that, if the circuit was not made of metals, or other very good conductors, the person who laid hold of them, in order to perform the experiment, felt a considerable shock in that arm which was in contact with the circuit. See *History of Electricity*, p. 95.

Lastly, in the course of my experiments with large electrical batteries, I found the force of this *lateral explosion* (as I shall chuse to call it) to be very considerable. For I several times observed that a chain communicating with the outside of the battery, but which made no part of the circuit, made a black stain on a piece of white paper on which it accidentally lay, almost as deep as the chain that formed the circuit (*History*, p. 644) and when, in order to judge, by

my feeling, of the *lateral force of electrical explosions*, I made it pass over a part of my naked arm, the hairs of the skin were all tinged, and the *papillæ pyramidales* raised, not only along the path of the explosion, but also where ever any part of the chain had touched it, though not in the circuit. *Ib.* p. 686.

It was to ascertain the nature and effects of this lateral explosion, that the following experiments were made; and in reciting them I shall distinctly note the progress of my own thoughts in the course of the investigation, from a state of absolute uncertainty, to that of the fullest satisfaction; and I flatter myself that some of the facts I shall exhibit will give pleasure to those who are best acquainted with, and most interested in the history of electricity.

Not having the least doubt but that, if any electric spark passed between a body that was insulated and another, the insulated body would appear either to have received or to have lost electricity, I imagined that nothing more was to be done than to insulate bodies placed
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within the influence of the electric circuit, with pith balls hanging from them, and upon their diverging with the electric spark, immediately to observe of what kind the electricity they had contracted was; and, previous to the experiment, I conjectured it would be negative; supposing that the discharge from the inside coating in an interrupted circuit was not able to supply the outside fast enough. And since the larger the insulated body was, the greater the quantity of the electric fluid it was capable of receiving, or parting with, and consequently the more sensible the effect would be, I began with suspending on silken strings a pasteboard tube covered with tin foil, seven feet long, and four inches thick, with large knobs at each end; and a brass ball at the end of an iron rod, which communicated with the outside of the jar, was placed within about a quarter of an inch of it, while the discharge was made through an insulated interrupted circuit, no part of which was less than two feet from the insulated tube.

On making the explosion, the spark appeared as I expected, but, to my great sur-

prize, I could not find that either positive or negative electricity was communicated to the insulated tube. Neither the pith balls, nor the finest threads, diverged or moved in the least, at or after the discharge, though, every thing else remaining in the same state, the least sensible electricity communicated to this tube (a quantity so small as hardly to be visible in the form of a spark at the time of communication) made the balls and the threads separate to a great distance, and would have kept them in a state of divergency more than an hour. Lest a small degree of motion, or divergency, should escape my notice, while I was intent upon making the discharge, I had an assistant along with me, whose eye was upon the threads all the time that I was making the experiment.

This experiment, as will easily be imagined, shook my whole hypothesis, and confounded all my ideas. I could not question the fact, having repeated the experiment with precisely the same event, I believe, above fifty times, on account of my having been hardly able to believe my own senses, or those of others.

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There was an evident electric spark, sometimes near half an inch in length, between the bodies composing the electric circuit and the insulated tube, in such a state of the air, as I knew, by frequent trials, would have kept it electrified a long time; and yet there was no communication of electricity.

I do not remember that I was ever more puzzled with any appearance in nature than I was with this; and various were the schemes that occurred to me of accounting for it, and the methods which I proposed to diversify it, in order to find out the cause of this strange phænomenon. Accordingly, I was no sooner at liberty to attend to this experiment, but, repeating it with some difference in the disposition of the apparatus, I observed that, upon every discharge, a slight motion was given to the threads that hung from the insulated tube. Upon this, the impossibility of of an electric spark neither *giving* nor *taking* any thing from an insulated body, contrary to my attentive observation, and that of my assistant, I concluded that some motion must have been given to the threads before; espe-

cially when I found that, in these latter experiments, the communicated electricity was always positive, the same with that of the inside of the jar. But the quantity of it was so small, that the most exquisite contrivance was necessary to ascertain the nature of it. For though, upon this occasion, the lateral spark was near a quarter of an inch in length, the threads on the insulated tube could only be made, by the explosion, to change their position from leaning a little one way to leaning as much the other, in the neighbourhood of an insulated brass rod, loaded with a small quantity of positive or negative electricity.

I could not help, however, being surprized that so large a spark should give no more electricity to the insulated tube than it appeared to have done; when, in other circumstances, a spark ten times less than this would have made a great and permanent alteration: Yet, improbable as these circumstances were, I entertained no doubt at that time but that these insulated bodies were electrified, either positively or negatively, according as the inside of the jar was positive or negative, by
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this lateral explosion ; though the *degree* was exceedingly small ; and I continued in this persuasion the longer, as it happened to be a considerable time before I had got another spark that communicated no sensible electricity.

I cannot help taking notice that, if it had happened that, in my first experiment, the insulated tube had always acquired or lost the least sensible electricity (and that I afterwards found there were many chances against the first result) I should have formed, and have acquiesced in some sort of hypothesis to account for the giving or receiving of electricity in those circumstances, and there the business would have ended. But the seeming contrariety of these appearances obliged me to pursue them farther.

Not being able completely to satisfy myself with my last conclusion, attended with the difficulties above mentioned, I kept diversifying the experiments, and introducing every circumstance that I could imagine might possibly affect the result of them ; and among the rest I made the following experiment,

ment, which quite unhinged me again, and left me as much at a loss as ever I had been before.

Having suspended a fine thread on an insulated brass rod, placed about one eighth of an inch from another rod, which was likewise insulated, and one end of which was in contact with the coating of the jar, and having electrified the rod which supported the pith balls, and placed a rod loaded with the same electricity near them; I observed that, upon every discharge, the balls which before were repelled were instantly attracted by the electrified rod; and that the result was invariably the same, whether they and the rod were loaded with positive or negative electricity; and also whether the jar was charged positively or negatively. I repeated the experiment several hours without the least variation in the event, which clearly proved that, in these circumstances, the electricity of the rod that received the lateral explosion was discharged by it.

Afterwards I repeated the experiment with some little variety, and found the electricity
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of the rod only lessened by the lateral explosion. These experiments, however, by no means favoured the supposition of the uniform communication of electricity, either that of the inside, or that of the outside of the jar; and, together with the former experiments, convinced me that this lateral spark by no means produced the effect that might have been expected in communicating electricity. But with the next set of experiments the difficulty began a little to clear up, and it continued to do so gradually, till I had gained all the satisfaction I could wish for with respect to this puzzling phenomenon.

The first time that I was able to vary the electricity of the insulated body placed near the electric circuit, or of the bodies that formed the circuit, which I now began to attend to, by any different adjustment of the apparatus, was on the following occasion.

Near to an iron rod that touched the bottom of a jar charged positively, I placed another insulated rod, with a pair of pith balls hanging to it, and observed, that when I attempted to make the discharge through an
imperfectly

imperfectly conducting circuit (bringing, *e. g.* part of the table into it) a strong spark passed between the insulated rod and the other that touched the jar; and immediately the balls separated as far as they possibly could, and continuing in a repulsive state, appeared to be electrified negatively. But immediately completing the circuit with good conductors, and making the remainder of the explosion in a full spark, another spark passed between the two rods, and immediately the balls fell close together again, and sometimes would separate with the opposite, *i. e.* positive electricity.

I could not, upon this occasion, make the lateral spark in the full explosion so great as in the imperfect discharge. I also observed that the more interrupted the circuit was, the farther would the lateral explosion reach; and that the electricity which the full explosion communicated was always positive when the jar was charged positively, and negative when it was charged negatively. The result of an imperfect discharge was always the reverse.

Insulating

Insulating several bodies, and the jar too, charged positively, they all equally contracted positive electricity by the discharge.

In this state of the experiments, I had no idea of the possibility of the lateral spark not communicating electricity to the insulated body; but I imagined that the kind of electricity communicated depended upon some circumstance in the disposition of the apparatus that I was not sufficiently aware of.

At length, recollecting that this last experiment resembled, in some respects, that curious one of professor Richman, mentioned in the *History of electricity*, p. 272, (in which it appeared that, when the coating of either side of a plate of glass communicated with the ground, the opposite electricity of the other side was more vigorous) I was satisfied that the negative electricity of the bodies that formed the circuit in the imperfect discharge was produced by the greater difficulty with which the outside of the jar was supplied, than the inside was discharged, so that the outside was comparatively in a state of insulation, and therefore would communi-
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cate negative electricity to all bodies within its reach; and from this I was led to conclude that, provided the jar was insulated, and the inside was made to part with its electricity with more difficulty than the outside received it, the bodies that formed the circuit would contract positive electricity, and the result answered exactly to my expectations.

I also concluded that, making the interruption in the middle of the circuit, since, in this case the inside would give, and the outside receive with equal difficulty, the bodies in the circuit, placed between the place of interruption and the inside of the jar, would be charged positively, and those placed between the place of interruption and the outside would be charged negatively; and this also was verified by experiment.

In this state of things, I found that I could give the insulated circuit what kind of electricity I pleased, provided there was any kind of interruption in some part of the circuit; and conjecturing that the electricity of bodies placed near the circuit would be the same with that of the bodies which composed
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fed it, I sometimes placed the rod which supported the pith balls near the circuit, and sometimes introduced it into the circuit, and found that, in both cases, it contracted the same electricity. This tended to confirm me in my supposition, that the lateral explosion was always attended with a giving or receiving of electricity, according to the nature of the circuit, and the place where it was situated; and I again overlooked the disproportion between the cause and the effect.

Presently after this, it occurred to me, that what may be called the redundant electricity of the outside or inside of the jar, separate from that which is *in* the glass, and constitutes the charge, must have some concern in this event, and the supposition was verified by fact. For insulating a jar charged positively, I observed that, when I touched the outside coating last (as is commonly done in setting it down) and made the discharge through good conductors, they were all electrified positively; and bodies placed near the circuit were the same. On the contrary, if, after placing the jar upon the stand, I touch-

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ed the knob of the wire, communicating with the inside, so as to take off all the redundant electricity, both the circuit and the neighbouring bodies contracted negative electricity.

I had at this time quite forgot that Epinus had made the same observation, on discharging a plate of air, mentioned in the *History of electricity*, p. 273; but considering what he says on that subject, I find he was mistaken with respect to the *reason* of this experiment not succeeding with Dr. Franklin and others, who had always asserted, that the electric circuit contracts no electricity at all by a discharge. For he says that the surfaces with which the Doctor tried the experiment were not large enough to make the effect sensible, and that the distance of the metal plates was likewise too small, as he says it necessarily must be, in the charging of glass; whereas I could give the insulated circuit as sensible an electricity with a common jar as he could with his plate of air; and much more depends upon the *height* of the charge, which must have been inconsiderable

ble in the plate of air, than the quantity of surface; which, however, may be increased at pleasure by multiplying jars in batteries.

I found, however, afterwards, that much depended upon the *quantity of surface* in the coating, and the bodies connected with them, as containing more of that redundant electricity, the effect of which was seen in the last-mentioned experiment. For when I discharged the jar standing in contact with the prime conductor, the tendency to the communication of positive electricity was so great, that, in that situation, the insulated circuit contracted strong positive electricity, when, every thing else remaining the same, except removing it from the conductor, and then making the discharge, it contracted no electricity at all.

Being now perfectly master of the electricity of the circuit in electrical explosions, and being able, in two different methods, to give which of the two electricities I pleased, I imagined that if I could so balance them, as to communicate neither, there would be no lateral spark, as in the above-mentioned

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experiments. But in this I was absolutely mistaken.

For, in the first place, when after setting the charged jar upon the stand, I took off, as near as I could guess, one half of the redundant electricity of the inside, and left both sides equally electrified (as appeared by the equal attraction of the pith balls to them both) the discharge of the jar, through a circuit of good conductors, did not, indeed, communicate the least sensible electricity to the circuit; but the lateral explosion was almost as manifest as before. The pith balls, hung upon the rod that received it, never separated.

In the next place I repeated this experiment by balancing the two different methods of communicating electricity to the circuit one against the other. For not insulating the jar, but setting it upon the table, which gave the circuit, and the bodies contiguous to it, an advantage for contracting positive electricity by the discharge; but, at the same time, making an interruption in the circuit, by introducing part of the table into
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it, which tended to give them negative electricity, I could easily manage it so, that the circuit contracted neither the one nor the other; and yet, as in the former case, the lateral explosion was as considerable as ever. The balls never separated.

To vary the experiment, I placed an insulated brass ball, two inches in diameter, round and smooth, so as not easily to part with any electricity it had got, in the place of the rod that supported the pith balls; and having found a situation in which no electricity was communicated to the circuit, I observed that none was communicated to it though, to all appearance, it received a spark of about a quarter of an inch in length. At least if it had contracted any, it was so little as to make it very problematical, whether a pith ball, or a fine thread, was moved by it, or not; whereas, when I gave it the smallest sensible spark, in any other manner, it would attract those light bodies for a long time together.

The interruption of the circuit I made use of in this experiment was not by means of

any part of the table, but only about a yard of brass chain introduced into it, and disposed between the inside jar, and that part of the circuit near which the insulated ball was placed. N. B. The ball must not be placed near the jar itself. For, in that situation, I found that, though it was very smooth, and perfectly spherical, yet it could not be placed very near the outside of the jar, standing on the table, without contracting negative electricity in a very small space of time.

These experiments threw me back into my former state of perplexity with respect to the lateral spark; since, when the two electricities of the circuit were exactly balanced, it was very little diminished, and yet the body that received it was not in the least sensibly electrified. But, upon reflection, I concluded, that this lateral spark must be of the nature of an explosion, and consequently that an electric spark must enter and pass out again, within so short a space of time, as not to be distinguished, and leave no sensible effect whatever. For though, in
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this case, part of the electric matter natural to the body must be repelled, to make room for the foreign electricity, its restoration to its natural state was so quick, that no other motion could correspond to it.

This hypothesis is favoured by the observation, that it is the very same thing whether a body be introduced into the circuit, or placed near it, with respect to contracting electricity, that is, whether the electric charge enter the body at one place and go out at another, or whether it be received or emitted at the same place.

This lateral explosion is an effect similar to a partial circuit, in which part of the electric matter that forms the charge in an explosion goes one way, while the rest of the charge goes another. The only difference is, that this detached part of the charge leaves the common track, and returns to it again, in the very same place.

Several remarkable partial circuits occurred in the course of my experiments before, particularly one mentioned in the *History of Electricity*, p. 692, in which part only of

the explosion passed in the shortest way, while another part of it took a circuit consisting of the same materials thirty times as long, and another mentioned, p. 691, where one circuit was made through a thick rod of metal, and another, at the same time through the open air.

That there is an admission and an expulsion of the electric matter in this lateral explosion, seems evident from this circumstance, that it is far more considerable when the body that receives it is large, than when it is small. In the former case, there is room for the electric matter natural to the body to retire upon the admission of the foreign electricity belonging to the charge, whereas, in the latter case, there is not room for it. When I placed a small brass ball, of about a quarter of an inch in diameter, near the circuit, I could not perceive that it was at all affected by any lateral explosion; and the spark was very inconsiderable when I placed a needle, about two inches in length, to receive it. But when I connected the large tube above mentioned, by means of a
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pretty thick iron wire, to any body whatever that was placed in the neighbourhood of the circuit, I have, with a jar of only half a square foot of coated glass, made the lateral explosion an inch or more in length, consisting of a very full and bright spark of electric fire. Insulated bodies of about eight or nine feet in length, seem to admit as large a lateral explosion as any body whatever is capable of. For, connecting them with the earth, by means of the best conductors, which gave the electric matter in the bodies the freest recess possible, I could never make this explosion much more considerable, using the same jar, and all other circumstances the same.

It is a manifest advantage in these experiments, that the lateral explosion be not taken from the coating of the jar itself, or from any part of the circuit very near to it. I have found that, *cæteris paribus*, it is the most considerable when it is taken at the extremity of a brass rod of one foot, or a foot and a half long, the other end of which is contiguous to the jar. It is analogous to this, that the

longest spark is taken not from the body of the prime conductor itself, but at the extremity of a long rod inserted into it. The electric matter seems to acquire a kind of *impetus* by the length of the medium through which it passes. But I found that the *maximum* in this case, did not exceed, or rather did not quite reach, three feet; for, making use of a thick iron rod, eight or nine feet long, the lateral explosion, taken at the extremity of it, was about the same as when it was taken at the end of a rod four inches from the jar, and not half so considerable as when taken at the extremity of a rod one foot long. This I imagined might be owing to the obstruction which the electric fluid meets with in passing even through metals, which appears, by my former experiments, to be much more considerable than was generally imagined.

Upon the whole, this remarkable experiment seems to be made to the most advantage in the following circumstances. Let the jar stand upon the table; let a thick brass rod, insulated, stand contiguous to the coating;

ing; and near the extremity of this rod, place the body that is to receive the explosion. This body must be six or seven feet in length, and perhaps some inches in thickness, or be connected with a body of those dimensions. Lastly, let the explosion be made with the discharging rod resting upon the table, close to a chain, the extremity of which reaches within about an inch and a half of the coating of the jar. In this case, the operator will hardly fail of getting a lateral explosion of an inch in length, which shall enter and leave the insulated body, without making any sensible alteration in the electricity natural to it.

With large jars, containing three or four square feet of coated glass, bearing a very high charge, I make no doubt, but that this experiment might be made to much more advantage. But at the time that I was engaged in this investigation I happened not to have any such jar, and therefore only used one that contained half a square foot of coated glass.

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If the interruption in the circuit, which is almost necessary in these experiments, be made by introducing a length of chain into it, rather than by making part of the explosion pass along the table there is a medium in the length of chain, that answers better than either a longer or shorter circuit. In a long interrupted circuit, the electric matter seems to lose the *impetus* which it discovers in a short one.

In all these cases, the electric charge seems to remain for a moment in the parts of the interrupted circuit, and therefore instantaneously rushes, in all directions, as well towards bodies that are not placed along its passage to the jar, as those that are; but, when the same charge occupies a larger circuit, it has more room to expand itself, and is not so strongly impelled to desert it. I found, however, by repeated trials, that when I made use of three yards of brass chain in the circuit, there was a distance to which the lateral explosion would not reach. The same distance it also would not reach when
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the circuit consisted of only one brass rod; but it reached it with great ease when only half a yard of chain was used, even without any other interruption in the circuit. But it reached to a much greater distance when the chain was very short, and the interruption was greater in other respects.

I had imagined, that, since the body which had received the lateral explosion, contained, for a moment, more than its natural quantity, that, if it were acutely pointed, some would escape, and that, upon the return of the explosion, the body would be exhausted. But I found no such effect, though I affixed fine needles to the bodies I made use of. The lightest pith balls, placed near the extremities of these needles, were not in the least affected by the explosion.

When I placed a number of brass balls, one behind another, the lateral explosion passed through them all; being visible in the intervals between each of them, and returned the same way, leaving them all in the same state in which it found them; and a great number of lateral explosions might
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be taken at the same time, in different parts of the circuit, some of them very near one another.

It made no difference whether the lateral explosion was received on a flat smooth surface, or the points of fine needles. In both cases, the spark was equally long, and vivid.

I had no sooner completed these experiments on the lateral explosion, but I had a curiosity to see what kind of appearance it would make *in vacuo*, since no other phenomenon in electricity resembles it. In all other cases, the electric matter rushes in one single direction; whereas, in this, it goes and returns in the same path; and, as far as can be distinguished, in the same instant of time; so that all the difference of the two electricities, which are so conspicuous in *vacuo*, must here be confounded. Accordingly, I found, though my pump was not in good order, that I could perceive this explosion in *vacuo* at the ends of rods placed several inches asunder; and when they brought within about two inches, they seemed to be joined by a thin blue or purple light, quite uniform

uniform in its appearance. As these rods were made to approach, this light grew denser, but still exhibited no such variety as is observed between the bodies that *give* and *receive* electricity, in the common experiments in vacuo.

I was pretty soon convinced, that uncoated jars could not be used to any more advantage in these experiments than those that were coated; since the want of coating only operated as an interruption in the circuit, occasioning a difficulty in the admission of the charge on the outside of the jar. And, in all cases, the greater this difficulty of passage was made, provided the discharge was made at once, the more considerable was the lateral explosion; and the greater shock was given to the hand that held the discharging rod, which shock was nothing more than one of these lateral explosions, issuing from the rod, as part of the circuit.

I shall conclude the account of these experiments with observing, that they may, possibly, be of some use in measuring the conducting power of different substances;

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since, the greater is the interruption in the electric circuit, occasioned by the badness of its conducting power, the more considerable, *cæteris paribus*, is the lateral explosion.

SECTION XXVIII.

Miscellaneous experiments in electricity.

1. *Experiments relating to the breaking of glass jars by electric explosions.*

FEW persons, I believe, have had so much experience in jars broken by electric explosions, as myself; having originally constructed very large batteries of very thin glass. In the history of my electrical experiments, I have mentioned an instance of six jars, containing each one foot of coated surface, bursting at one explosion. Since that time, having discharged a battery at Leeds, in the presence of Dr. Franklin and Mr. Canton, though I perceived nothing particular, and suspected no accident, at the time

time (the full force of the explosion having, to all appearance, been received in the usual manner) yet when I was about to charge it again, I found that no less than *ten* jars had been broken at that one time. It was some consolation to me, however, that the accident happened in the presence of two such eminent electricians.

Having been so great a sufferer in this way, and having always such a number of broken jars at hand, I could not be without thinking of expedients to repair them; and nothing *a priori* promises so well to answer the purpose, as such *cements* and *varnishes*, as are known to be impermeable to the electric fluid. But though these cements and varnishes will sometimes answer pretty well with *thick* jars, I have not yet found any method of effectually repairing *thin* ones, such as mine have generally been; and I do not know of any facts that ever puzzled me more than the following, which occurred in my attempts to repair them.

I have found invariably, so that, extraordinary as it may seem, it is impossible that I
can

can be mistaken with respect to it, that whenever I had covered the fractured place of a jar with any kind of cement, or varnish, (and particularly an excellent *amber varnish*, recommended to me for that purpose by my then tutor in chemistry, Mr. Turner of Liverpool, and which hardened into a substance as firm as glass) the jar never failed to break again at the very next charge, and generally before it had received half its proper charge. But what is most remarkable, is the following circumstance, that the new fracture was never made in the place of the old one, but always exactly in some place where the cement terminated. There I found a new perforation of the glass, and a new fracture, which had no communication with the former. To whatever distance I extended this new coating of cement, the event was the very same. For the fracture never failed to happen, and exactly in some part of the glass, where the cement ended.

Seeing in so many instances, that the new fractures had no connection with the old ones, but that they always happened at the
termination

termination of the cement, I concluded that the coating of cement must have been one principal cause of the fracture ; and therefore I repeated the experiment with a jar that had *not* been broken. And, in order to be quite sure that the expected fracture, if it should happen, might appear to be caused by the coating of cement only, I took a phial, and coating it inside and outside, in the usual way, I found that it bore a full charge very well. For my greater satisfaction, I charged it and discharged it several times.

Having in this manner ascertained the strength of the jar, I took off a little of the outside coating, and put on it a small patch of cement, about an inch in diameter. Then, drawing the former coating over it, I proceeded to charge the phial as before. But before it had received half its full charge, it burst by a spontaneous explosion, not indeed at the termination of the cement, as in all the cases above referred to, but in the middle of the patch, where it happened to be exceedingly thin, much thinner than near the extremity of the patch.

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I then

I then covered another phial entirely with cement, and after coating it inside and outside, in the usual way, proceeded to charge it as before; when this phial likewise burst, and in a place where neither the glass nor the cement was particularly thin; the cement being of the thickness of a shilling, and the fracture happening near the bottom of the phial; where the glass is generally pretty thick.

Lastly, I covered a phial both inside and outside with cement, and after that coated it inside and outside with metal, in the usual way; so that all the glass seemed to be guarded from any accident. But, notwithstanding this, it burst at the very first attempt to charge it.

I expected that, by covering the whole phial with cement, so that there was no place of termination, where the new fractures had generally happened, the jar would be more secure. But I found to my cost, that even an entire coating of cement was no more safe than a partial one. Why a glass jar should be endangered by an *electric coating*, which cement
is,

is, I cannot conjecture; but I propose, at my leisure, to diversify this experiment, and investigate the cause of it, if I can.

2. *Of the supposed nonconducting powers of water and quicksilver, in the state of vapour.*

In my last publication I gave an account of some experiments, which seemed to prove that steam, or the proper vapour of water, and even of quicksilver, were nonconductors of electricity; because the electric matter passed through them both in a *full spark*, exactly as it does in air, which is known to be a non-conductor. I concluded that, had the electric matter passed in the substance of the vapour itself, so as to be properly conducted by it, it would have passed invisibly, as it does in metals or water, &c. But in repeating these experiments with some variation, I have since been led to conclude, that, though the electric matter does pass through these vapours, to all appearance, exactly as it does through a body of *air*, yet they are not capable

ble of confining the electric matter, so as to insulate electrified bodies, as air is.

These vapours may, however, in reality, be nonconductors of electricity, and it may be owing to the *heat*, which is necessary to the preserving their form of vapour, that they do not insulate. For I found that, in the same degree of heat, even the glass which contained them would not insulate, and that it was pervious to the electric explosion, without any injury to it. Though the experiments are, therefore, inconclusive, the probability *a priori*, is still, I think, in favour of the opinion, that every substance in the form of air is, when cold, a nonconductor, as air itself is.

Beginning where I had left off, I filled a glass syphon with mercury, and putting each of the legs into separate glass cups of mercury, I placed the upper, or the bent part of the syphon, near the mouth of a small furnace, that the mercury in that place might be turned into vapour, and the running mercury might descend into the legs of the syphon. Then applying the rod of a charged jar

jar to a brass ball, connected with the mercury in one of the glass cups, I found that it could not retain electricity the smallest space of time, though the mercury was completely insulated; so that the electric matter which I communicated to it must have passed both through the vapour and the glass itself. In this case, however, the electric matter was transmitted invisibly. But when I made this syphon part of an electric circuit, the electric matter passed visibly through the vapour, in the manner described before.

I then introduced only one leg of the syphon into the circuit, and the explosion passed freely through the vapour, and the glass, at the place where it was hottest. It was visible in the vapour, but divided into several streams towards the top of the glass, and then passed invisibly through the hot fire, the space of several inches. That it did take this road, was evident by making interruptions in the circuit, which consisted of pieces of metal lying on the floor, beyond the fire. But when the circuit was much interrupted, I always perceived that part of the

explosion went round to the quick-silver in the other leg of the syphon, though it was completely insulated, and there was no passage for it that very way into the air. This, therefore, must have been a case of the *lateral explosion*, which passes and returns at the same instant, of which I gave a particular account, in a paper inserted in the *Philosophical Transactions*, and which is contained in the last section.

Laying aside the syphon, I filled a glass tube, closed at one end, with mercury; and then inverting it, and heating the upper part of it, while the lower part was plunged in a glass cup of mercury, I had the same results as with the syphon; the mercury, so insulated, not being capable of retaining electricity, and the explosion passing quite freely through the vapour and the glass itself.

When I placed a brass rod very near the top of the tube, and made it part of the circuit, I found that the explosion passed through the vapour, and the substance of the glass, to come at it; but I could not
perceive

perceive any spark between the glass and the rod, though the electric matter passed visibly through the vapour within the glass. This seems to shew that something within the fire, in the space between the glass tube and the brass rod, was a proper conductor of electricity, as it passed invisibly in that place. But that the electric matter found some resistance within the glass, is, I think, evident, from its passing visibly there, just as it does in all kinds of air.

SECTION XXIX.

Of sound in different kinds of air.

ALMOST all the experiments that have hitherto been made relating to *sound*, have been made in common air, of which it is known to be a vibration, though it is likewise known to be capable of being transmitted by other substances. There could be little

doubt, however, of the possibility of sound *originating* in any other kind of air, as well as of being *transmitted* by them; but the trial had not been actually made, and I had an easy opportunity of making it.

Besides, the experiments promised to ascertain whether the *intensity* of sound was affected by any other property of the air in which it was made than the mere *density* of it. For the different kinds of air in which I was able to make the same sound, besides differing in specific gravity, have likewise other remarkable chemical differences, the influence of which with respect to sound would, at the same time, be submitted to examination.

Being provided with a piece of clock-work, in which was a bell, and a hammer to strike upon it (which I could cover with a receiver, and which, when it was properly covered up, I could set in motion by the pressure of a brass rod, going through a collar of leathers) I placed it on some soft paper on a transfer. Then taking a receiver, the top of which was closed with a plate of brass, through which
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the brass rod and collar of leathers was inserted, I placed the whole on the plate of an air pump, and exhausted the receiver of all the air that it contained. Then removing this exhausted receiver, containing the piece of clock-work, I filled it with some of those kinds of air that are capable of being confined by water, by means of a bent glass tube inserted into a piece of brass, which I could screw into the bottom of the transfer, so as to introduce the bended tube, through the water of my trough, into a jar containing the air on which I wished to make the experiment. For a description of this apparatus, see vol. i. pl. 2. fig. 14.

When this was done, I removed the glass tube, and then I had the receiver filled with that species of air in which I wished to produce the sound, and the apparatus for making the sound within it. Then by forcing down the brass rod through the collar of leathers, I made the hammer strike the bell, which it would do more than a dozen times after each pressure. And the instrument was
contrived

contrived to do the same thing many times successively, after being once wound up.

Every thing being thus prepared, I had nothing to do, after filling the same receiver with each of the kinds of air in its turn, but receding from the apparatus, while an assistant produced the sound, to observe at what distance I could distinctly hear it. The result of all my observations, as far as I could judge, was that the intensity of sound depends solely upon the *density* of the air in which it is made, and not at all upon any chemical principle in its constitution.

In inflammable air the sound of the bell was hardly to be distinguished from the same in a pretty good vacuum; and this air is ten times rarer than common air.

In fixed air the sound was much louder than in common air, so as to be heard about half as far again; and this air is in about the same proportion denser than common air.

In dephlogisticated air the sound was also sensibly louder than in common air, and as I
t hought

thought rather more than in the proportion of its superior density ; but of this I cannot pretend to be quite sure.

In all these experiments the common standard was the sound of the same bell in the same receiver, every other circumstance also being the same ; the air only being changed, by removing the receiver from the transfer and blowing through it, &c.

SECTION XXX.

Miscellaneous experiments.

1. *Of lime water in a solution of iron with spirit of nitre.*

I HAD discovered (see vol. iv. p. 288) that the cause of the change of colour from blue to red in the calx of iron, is the dephlogistification of it ; tracing the progress of the phlogiston through a large body of water, at the bottom of which the precipitate of the iron lay, into the air above it, which it phlogi-

phlogisticated. I then produced the blue precipitate, by pouring a diluted solution of fixed alkali into a solution of green copperas. But I have since accidentally met with a much better method of making the experiment, by means of lime water.

The trough in which I make my experiments on air being at one time very foul, with various metallic solutions, especially in consequence of having dissolved iron in spirit of nitre, for the production of nitrous air, and other purposes, and it not having been convenient to change the water, I continued to use it in that state; when casually pouring a little lime water into it, I observed that a precipitate of a very deep blue colour was formed. It was so beautiful, that, having been obliged to leave my experiments, for the sake of a small excursion to Bath, before I saw any farther into it, I remember telling a friend whom I met there, that I thought it possible that I had accidentally discovered a new and cheap method of making Prussian blue. However my dream of a discovery vanished on my return home, when I observed the bottom of my trough
covered

covered with a very lively *red*. But when I turned it up, I found the red was only superficial, and that the precipitate underneath was of as deep a blue as ever.

I then repeated the experiment in small jars, phials, &c. and was much better pleased with the result than when I had made use of a solution of alkali in order to make the precipitate. Here the lime is seized by the acid, as it was before by the alkali; and in both cases the calx of the iron is set at liberty, and deposited in a phlogisticated state. But it readily parts with its phlogiston if pure air be at hand, even though separated from it by a body of water, of I believe any depth. These experiments shew that water, though capable of receiving phlogiston, is not capable of retaining it in the presence of air, which appears to have a much stronger affinity with it.

2. *Of an unexpected appearance of volatile alkali.*

Having, for the purpose of producing a large quantity of that kind of nitrous air in
which

which a candle burns with an enlarged, or with a vivid flame, filled a large jar with pieces of iron wire, and having repeatedly poured upon them a diluted solution of copper in the nitrous acid, at length a thick incrustation was formed upon them; and having no occasion to make use of the jar for several months, I took no notice of it till I found the jar was burst by the swelling of that saline incrustation.

The substance of this matter was generally red, being the calx of iron; but there was mixed with it a quantity of *green matter*, which, when broken had a strong smell of volatile alkali. I then doubted whether this arose from any of the materials I have mentioned, or from something else which had got into the jar, unknown to me. If the former were the case, which, however, at that time, I could hardly suppose, I thought it to be not a little remarkable; but I have since had another opportunity of observing the same fact; having examined a second jar filled with iron wire, which had been treated in the same manner, and found the same strong smell of volatile alkali.

alkali. Also I now the less wonder at this fact, which puzzled me so much at the first, as I find, in Mr. Keir's very valuable notes to his translation of Mr. Macquer's chemical dictionary, that volatile alkali has been found in many earthy substances, and amongst others, in *rust of iron distilled*.

In this case, the calx of the iron being supersaturated with phlogiston from the nitrous air, decomposed by it, the alkali, of which this and other metallic calces consists, uniting with it, becomes volatile alkali.

It should seem that, in general, the calces of metals contain less phlogiston than the metals themselves; and for this reason I was originally led to conclude, that nitrous air exposed to iron, which is evidently turned to *rust*, or a *calx* in it, had received phlogiston from the metal; and I therefore termed the nitrous air that had been so treated *phlogisticated nitrous air*. I now think it most probable that this rust of iron contains more phlogiston than the iron itself, and that the nitrous air, in which, after this process, a candle burns better than in common air, is properly

properly termed *a dephlogisticated nitrous air*, having parted with its phlogiston to the iron.

I expect to find, in my future experiments, that it may not be difficult to determine whether this rust of iron contains more or less phlogiston than the iron itself. The former I own I strongly suspect, and therefore that it differs much from common rust of iron. For I am far from being disposed to question the truth of the common opinion, that metals consist of phlogiston and a peculiar earth.

3. *Of air not being sensibly injured by offensive putrid substances.*

It has been observed both by myself and others, that air exceedingly offensive to the nostrils is not always properly phlogisticated, so as to be distinguished by the test of nitrous air. For though it may be true that phlogiston is the thing that constitutes *smell*, or at least that it is in some manner essential to it, that phlogiston which sensibly affects the olfactory nerves may be attached to something

thing that is only *diffused* through the air, and not properly *incorporated* with it. For when this air, so exceedingly offensive to the nostrils; is made to pass through a body of water, this phlogiston is entirely separated from it, and leaves the air through which it was diffused, and which it had seemed to contaminate, quite pure and inoffensive.

In order to make full proof of the truth of this observation, and also with the farther view, of trying whether the quantity of phlogiston contained in an animal substance might be so far exhausted by putrefying in quicksilver, as to be unable to phlogistificate common air, I confined a large piece of the tendon of a neck of veal, and likewise a whole mouse, in separate vessels of quicksilver, some time in September, 1779; and when they had yielded all the air that I could perceive they would yield, and of which an account has already been given in a former section, I took what remained of them both in the April following, and putting them into a jar of common air, containing about seven ounce measures, I examined this air after

two days, and did not find it sensibly injured, though the substances were very offensive to the nostrils. After keeping them, however, in the same jar about two months longer, I found the air to be phlogisticated.

Notwithstanding this, I make no doubt but that in *length of time* these substances would have lost all their power of phlogisticating air. But whether this property, or that of yielding an offensive smell, would have gone first, I had no opportunity of observing, in consequence of removing my habitation, by which I was obliged to put an end to the process. It appears, however, sufficiently, that very much of the power of these putrefying substances to phlogisticate air was gone before they ceased to be offensive, though it is probable they were not so highly offensive as they had been before.

SECTION XXXI.

Remarks on certain passages in the preceding volumes of my Observations on Air, explaining, or correcting them, by the help of subsequent experiments and observations.

N. B. The first number denotes the *page*, and the second the *paragraph*. When no paragraph is expressed, the *first* in the page is to be understood.

VOLUME i. page 38, paragraph 2. Inflammable air burns blue when it is mixed with fixed air. The inflammable air, in this case, came, I doubt not, from the *iron*, which I afterwards found to yield inflammable air by heat only. See vol. ii. page 107.

P. 42. The addition of permanent air, in this case, came, I doubt not, from the iron filings and brimstone; which, in time, even in the temperature of the atmosphere, yields a quantity of inflammable air, which is lia-

able to be changed afterwards, and often presently, into phlogisticated air, as will be seen in the present volume, p. 83, &c.

P. 45. If fixed airmakes a part of the constitution of common air, it should seem that it ought to be deposited when brimstone is burned over it, as well as when other substances are treated in the same manner. For though the acid of vitriol may unite with the lime, in the lime water over which it is burned, it would hardly make part of the same substance with, at least, any considerable portion of fixed air; because the stronger acid would expel the weaker, if it had been there before.

In some saline substances, as *alum*, there are both vitriolic acid and fixed air, but the latter is in small quantity.

P. 56. More air is procured from any substance by a quick than by a slow process, and nitrous air continuing a long time unabsorbed by water becomes less capable of being absorbed, as will be seen in the present volume, p. 129. In both these cases, the influence of *time* is very remarkable.

P. 57, 2. I

P. 57, 2. I suspect that this ochre, and these flowers of zinc, were produced by some part of the solution of those metals mixing with the water in the trough; after which the phlogiston escaping into the air, the calces were precipitated; and that this ochre had never been incorporated with the air, at least no more of it than was perceived to make it turbid, when it was first produced. But I still think it probable, that some earth, and of course earth from the metals, forms the basis of inflammable air.

P. 59, 2. It is possible that in pure water, inflammable air might not be changed into phlogisticated air, though I cannot tell what kind of impregnation in the water promotes this change. Urine will do it, as may be seen in this volume, p. 129.

P. 61, 2. Since the experiments on the willow plant, recited in this volume, I rather think that the diminution of inflammability in this quantity of air was owing to the growth of the plant in it.

P. 65, 4. The case of the inflammable air firing with one explosion in the vapour of spirit

of nitre is similar to a candle burning with a vigorous flame in nitrous air exposed to iron, &c. It contains a vapour, or a species of air, that is capable of being absorbed by water. This vapour, &c. is capable of taking phlogiston from burning bodies, though it will not support animal life. But they differ in this, that this nitrous vapour which inflammable air takes immediately from spirit of nitre, is instantly absorbed by water; whereas that which is produced from nitrous air, as well as in some direct processes, by means of nitrous acid, is capable of being transferred through water many times, and is so combined with some other principle, as not to discover any mark of acidity, any more than nitrous air itself, and in some cases less than nitrous air.

P. 69, 2. Water will take more or less of inflammable air, as well as of other kinds of air, in proportion to the quantity of air which it contains already.

P. 84, 3. Six ounce measures of phlogistified air were produced in this case from a single mouse putrefying in water; whereas if
it

it putrefies in quicksilver, there will not be a single ounce measure of such air procured. This is a subject that deserves to be investigated farther.

P. 99. As phlogisticated air is common air loaded with phlogiston, though fixed air be an acid, and, like other acids, has some affinity with phlogiston; yet the basis of common air (which appears to be an acid principle common to the nitrous and vitriolic acids) has a stronger affinity with it; and therefore it is not in the power of fixed air to deprive the common air of the phlogiston which is incorporated with it, so as to improve that air.

P. 103, 2. It is most probable, that fixed air in the bowels strengthens the tone of them, and thereby enables them to expel the putrid matter, and that it does not properly, as I imagined at first, unite with the putrid matter, and thus render it less offensive. In this manner, too, it is probable, as Dr. Millan informs me is his opinion, that fixed air acts in the stomach, as a medicine for the sea scurvy.

P. 106. This smell of volatile spirit of vitriol must have arisen from vitriolic acid air, produced by the union of the acid of vitriol contained in the sulphur, with phlogiston, coming probably from the iron.

P. 107. That inflammable air must have been in a state of diminution by standing in water. For I have since found that, with long standing, this mixture produces air, and generally inflammable air, even in the temperature of the atmosphere. See p. 107. of this volume.

P. 114, 2. The nitrous acid will unite with much of the lime, with which it makes a salt, that is perhaps in a considerable degree soluble in water. However, many experiments, and especially some recited in this volume, make it doubtful whether there be any fixed air properly incorporated with common air, so as to make a constituent part of it.

P. 121, 2. I do not yet perfectly understand the nature of this filmy matter collected from my trough.

126, 2. After-

P. 126, 2. Afterwards air considerably nitrous was procured from lead. See vol. ii. p. 173.

P. 127. Here was an instance of the calx of zinc absorbing spirit of nitre, as described in this volume, p. 237, &c.

P. 128, 2. In vol. 3. p. 165 and 166, it will be found that I procured different quantities of nitrous air from iron. To ascertain this quantity with accuracy, more attention should be paid to the quantity of phlogiston both in the nitrous acid, and the iron itself, as several chemists have observed that this metal varies much in this respect. I propose some time or other to repeat these experiments with an attention to more circumstances than I was apprized of in this early stage of the business.

P. 135, 2. The yellow tinge of the water over which the metals were calcined arose probably, from the calces of the metals, and the *smell* from part of the phlogiston set loose in the process.

P. 137. This white powdery substance could be nothing but the calx of the metal.

P. 138, 2. It

P. 138, 2. It will appear by future experiments, that the oil contributes to phlogistificate the air more than any other ingredient of the paint.

P. 153, 2. The marine acid air must have seized upon the water, and have left it in the state of calcined alum.

P. 154, 3. The effect of the proper acetous fermentation of air deserves to be examined with attention.

P. 155, 3. This is the first dephlogisticated air that I procured. The note was written when I thought it was dephlogisticated nitrous air that I had got.

P. 156, 2. As I never had any instance of dephlogisticated air becoming thoroughly noxious, and being restored so soon, I suspect there must be some mistake in this place. But it is of little consequence now, since the nature and properties of this kind of air are fully ascertained.

P. 171. 4. I do not know what the white cloud mentioned in this place can have been.

P. 175, 3. I suspect that the inflammability of this air came from some thing mixed with

with the alkaline air, and not from any thing essential to it. My reasons will be seen in the present volume; p. 224.

P. 179. *Note.* Nitrous air phlogisticated with liver of sulphur should be struck out from this note, as the air to which I there allude appears now to be dephlogisticated, and not phlogisticated.

P. 186, 3. The quantity of solid matter formed by taking the electric spark over lime water was so small, that the experiment ought to be repeated on a larger scale, in order to examine what kind of substance it really is.

P. 192, 2. It could not be a real, but only a seeming calcination, or a dispersion of the tin, that was made in a close vessel.

P. 193, 2. Had I used more heat, dephlogisticated air would have been produced, as well as fixed air.

P. 214, 3. Did not the fixed air in this place come from a slight tendency to putrefaction in the bladder?

P. 216, 2. The power of nitrous air after it has been exposed to iron &c. to diminish
common

common air, is explained in the present volume, p. 203.

P. 218, 2. The theory of this diminution of nitrous air is erroneous, as it is probable from subsequent observations, that the iron instead of losing phlogiston gains an addition of that principle from the nitrous air, which is thereby dephlogisticated.

P. 219, 2. Afterwards I found that, after a certain period in this process, a dephlogisticated nitrous air was produced. See vol. iii, p. 140.

P. 220, 3. Having never since found that nitrous air, without agitation in water, is diminished by fresh nitrous air, in consequence of exposure to iron, &c. I conclude that I must have made some mistake with respect to this experiment.

P. 249, 2. This remaining air I now find comes from the iron filings and brimstone, which first yields inflammable; whereas liver of sulphur seldom gives any.

P. 250, 3. It will be found that when this experiment is made with more accuracy, these different kinds of air expand unequally
with

with the same degrees of heat. See vol. iii, p. 347.

P. 252. The vapour of ether mixes with the air, and for a time assumes the form of air; but it is capable of being imbibed by water.

P. 254, 3. This substance containing nitre yielded dephlogisticated air, by the help of which the substance could burn; but in vacuo the dephlogisticated air was too much dilated, at the moment of its generation, to sustain any fire.

P. 260, 2. In this place I supposed heat to consist in a vibratory motion of the particles of bodies; and sensible heat probably does consist in, or is accompanied by, such a motion. But there may be a *principle of heat* latent in bodies, and not manifest by any sensible effect. Heat may therefore be what is usually termed a *substance*, whether it have the property of *weight*, that is, whether it be subject to the action of gravity, or not.

P. 266, 2. The experiments recited in this volume prove that vegetation increases the quantity of the air which it purifies.

P. 271, 2. It

P. 271, 2. It is dephlogisticated air that is formed in the accension of gunpowder, and which enables the other materials to burn with the violence which is peculiar to that composition.

Ib. 3. It appears from subsequent experiments, that it is not the marine acid that is the basis of common air, but an acid principle that is common to the vitriolic and nitrous acids. But though it should be a proper nitrous acid that is the basis of common air, an earth is also essential to it; and this chemical compound of nitrous acid and earth, may have a stronger affinity with phlogiston than the nitrous acid alone has, and therefore may seize upon it, so that the nitrous acid in the nitrous air may be precipitated by this means.

P. 273. It must be phlogiston that is taken from nitrous air when it is exposed to iron or liver of sulphur, if not also when the electric spark is taken in it, because it is left in a dephlogisticated state. But it may be difficult to trace the progress of the phlogiston which it has lost in all these cases.

P. 279.

P. 279. In Mr. Elliot's writings will be found some very ingenious conjectures, and pretty well supported, concerning the manner in which it may be supposed that muscular motion is performed, and the influence of phlogiston in this business.

P. 281. See the remark on p. 260.

Remarks on the second Volume.

P. 8. 2. I found afterwards (see vol. iii. p. 360.) that vitriolic air would not dissolve ice, and therefore I conclude that, in this case, a little moisture might adhere to the ice, which, unperceived by me, might imbibe the air. I had the less suspicion in this case, from having found that marine acid air, fluor acid air, and alkaline air dissolve ice. So that the property of vitriolic acid air, *not* to dissolve ice, is a remarkable exception to what may be called a general rule.

P. 171. I imagine that this nitrous vapour seized upon the phlogiston of the nitrous air, and thereby decomposed it, in the same manner as the nitrous acid itself will do.

P. 231,

P. 231, 2. It will be seen that a small quantity of nitrous acid, from the decomposed nitrous air, must have mixed with this water. See the present volume, p. 141.

P. 232. That alkaline air will not dissolve copper may be owing to its being already saturated with phlogiston, though when combined with water it does dissolve this metal. This compound, of alkaline air and water, may have quite different properties from the alkaline air alone.

1b. Caustic alkali may require phlogiston to assume the form of air, and it may not be easy to find any substance that has a less affinity with it than this alkali. Or the alkali may have a stronger affinity with water than the phlogiston.

Remarks on the third volume.

P. 26. It is very possible that the water in which this experiment was made might contain some fixed air, which, as I have observed, is readily communicated to any kind
of

of air that is made to pass through it. See vol. iii, p. 355.

P. 42. The experiments made with red precipitate make it probable, that by far the greatest part of the air consists of spirit of nitre, or rather of an acid principle common to it and oil of vitriol; since all the mercury may be revived from red precipitate, except about one twentieth part.

P. 54, 2. The nitrous acid in the solution of copper from which the nitrous air is extracted is not taken into the account of this process, and therefore there may be much less nitrous acid in four ounce measures of nitrous air than in forty-two of dephlogisticated air.

P. 102, 2. I afterwards found air in the stalks of some plants to differ in quality from the external air. See vol. iv. p. 313, &c.

P. 128, 2. It is imagined that nitrous acid combined with water has a stronger affinity with phlogiston than nitrous acid in the form of vapour, because it seizes upon the phlogiston of nitrous air, and decomposes it.

P. 139, 2 I have now no doubt, but that the former of these suppositions is the true one, or that this kind of nitrous air in which a candle burns contains a redundancy of nitrous vapour, diffused through the air, though so combined with some other principle, as not to shew any mark of acidity, when imbibed by water, &c. See this volume, p. 133, 203.

P. 146. The nitrous acid vapour must, I should imagine, be combined with some portion of phlogiston, if not with some other principle, not to give acidity to water.

P. 165. I have since found that nitrous air contains just the same quantity of phlogiston with inflammable air, bulk for bulk. See vol. iv. p. 378.

P. 201. This copious production of nitrous from water impregnated with nitrous vapour is a fact of a very remarkable nature, and deserves to be farther attended to.

P. 230, 4. The calx of lead, like the calces of other metals, imbibes spirit of nitre, and a little water. They all emit them again with heat. See the present volume, p. 241.

P. 267.

P. 267. The increase of the quantity of inflammable air from agitation in oil of turpentine, may be explained by my having found that oil of turpentine sometimes contains a considerable quantity of inflammable air, which may be expelled by heat. See vol. 4. p. 363.

P. 298. I have since discovered that the colour of spirit of salt is owing to some earthy matter dissolved in it. See vol. 4. p. 78, &c.

P. 350, 3. If, as is now said to be discovered by professor Bergman, spirit of wine consists of fixed air combined with the acid of sugar, this conclusion is not just. There are processes, however, which I still think prove fixed air to be a factitious thing, especially the phænomena of the phlogification of common air, because the diminution is completed when inflammable air is fired in it, without any appearance of fixed air.

Remarks on the fourth volume.

P. 77. The reason why the two quantities of air occupy less space when they are

made to pass up the tube slowly, is that thereby the water has a better opportunity of absorbing the nitrous air. See p. 180 of this volume.

P. 254. It will be found that the diminution of this quantity of dephlogisticated air did not proceed any farther in another whole year. See p. 154 of this volume.

P. 335. When I wrote the former part of this section, I concluded, and rightly, that the dephlogisticated air was produced by the *green matter* in the water. But when I wrote the second part, I imagined it to be produced by the influence of light upon the *water* itself disposing it to deposit the green matter. It will be seen in this volume that, having found this matter to be a plant, I presently satisfied myself, that this, and all other plants, are capable, by means of the action of light upon them, to depurate the air to which they have access, and thereby increase the quantity of it.

P. 458. It will be seen in this volume, p. 83, &c. that there was an addition of inflammable air from the iron filings and brimstone in this process.

S E C-

SECTION XXXII.

*A summary View of all the most remarkable
Facts in this and the four preceding Volumes.*

PART I.

Facts relating to COMMON air.

COMMON air is not affected by stagnation, i. 161, or by the crystallization of nitre, 161, 2, by the perspiration of the body, iv. 275, v. 104, or by steam, iv. 281, v. 135.

Air extracted from pure water is generally purer than atmospherical air, v. 168, 170.

Many kinds of effluvium mix with it, but do not incorporate with it, i. 157, 2.

Common air is phlogisticated and diminished by charcoal, i. 129, 2, by the calcination of metals, 133, by paint, 138, by liver of sulphur, 179, 2, by Homberg's pyrophorus, 179, 3, by the firing of gunpowder, 179, 4, by cement made with bees wax and turpen-

tine 179, 5, by iron which has been exposed to nitrous air, 181, 2, 222, by the electric spark, iv. 284, by nitrous ether, ii. 330, by the conversion of the blue calx of iron into red, iv. 289, by the solution of copper in volatile alkali becoming blue, 288, by water fresh distilled, 293, and by flowers, 311.

Fishes phlogistificate the air combined with the water in which they live, iii. 341. v. 136. They die in water impregnated with phlogisticated air, 138.

Common air is diminished by candles burning in it one fifteenth, or one sixteenth of its bulk. i. 44, It has by this process received about one third of the phlogiston that it is capable of receiving, 116, 2.

The diminution of common air by iron filings and brimstone is between one fourth and one fifth of the whole, i. 105.

It may be repeatedly diminished by nitrous air, and again cleansed by agitation in water, till the whole would disappear, i. 190, 2.

Common air is both phlogisticated and absorbed by oil of turpentine, iii. 94. Whenever it is phlogisticated it is probable that
part

part of it is absorbed, 97. It is liable to be absorbed by water, and the remainder is partially phlogisticated, i. 158, 2.

Common air is improved by the growth of plants, iv. 300, 307. It is also improved by being incorporated with water, and kept there some time, 353.

P A R T II.

Facts relating to DEPHLOGISTICATED *air.*

Dephlogisticated air may be extracted by heat from nitre, i. 155, v. 143, from alum, i. 155, from precipitate per se, ii. 34, from minium, 37, from manganese, iv. 203, and from lapis calaminaris, 206.

It is found in the bladders of sea weed, iv. 313, in water, 354, 466, in sea water, 356, 469.

It is produced by a green vegetable matter in water, iv. 338, but not without the influence of light, 342, 489, v. 18.

It is extracted by heat from red precipitate, ii. 35, from spirit of nitre and any kind

of earth, 55. The same earth may be used repeatedly with fresh spirit of nitre, till it vanish, 56. It is produced in the greatest abundance by the metallic earths, ii. 63, after these by the calcarious, ib. from minium by spirit of nitre, 53, from the earths of all the metals, iii. 6, from all other kinds of earth, 28. The quantity of it depends upon the quantity of spirit of nitre made use of in the process, ii. 378.

Dephlogistified air may be extracted by heat from green vitriol, iv. 213, from the other metals dissolved in vitriolic acid, 226, from blue vitriol, 227, from white vitriol, 228, from turbith mineral, 230, from earthy substances united with vitriolic acid, 236, from alum, 237, from quicklime and oil of vitriol, 238.

Dephlogistified air cannot be procured from any earthy matter dissolved in spirit of salt, iv. 240; but it may be procured from spirit of salt impregnated with red minium, which would yield it of itself, 442; but not from the same acid impregnated from the same minium after the redness had been taken away

away by a previous *affusion* of the same acid, ib.

An extremely pure kind of dephlogistified air is procured from mercury dissolved in spirit of nitre, iv. 246.

Dephlogistified air is heavier than common air, ii. 94. It is purer than common air, or fitter for the combustion of inflammable substances, and for respiration, ii. 39, &c. 48, &c. It even serves for respiration longer than the degree of its purity, as indicated by the test of nitrous air, would lead us to suspect, v. 156. Pyrophorus is fired in it, iv. 259. It is unfavourable to the growth of plants, iii. 336, iv. 326, v. 13.

When mercury is dissolved in spirit of nitre, and dephlogistified air is afterwards extracted from it by means of heat, the whole of the mercury cannot be revived, iv. 260.

There is no acidity in this kind of air, nor in the residuum of red lead, out of which it was extracted, ii. 373, observed by Mr. Magellan.

Dephlogistified air contributes to the easy formation of precipitate per se, v. 152.

P A R T III.

Facts relating to PHLOGISTICATED *air.*

Phlogisticated air is produced by charging common air with phlogiston, i. 138, by the nitrous acid with animal substances, ii. 146.

It is lighter than common air, i. 46. It differs in this and other respects from fixed air, *Philosophical Empiricism*, p. 42.

Phlogisticated air is restored by vegetation, i. 49, 87, iv. 299, 305, v. 12. By this means provision is made for lessening the effects of putrefaction in hot countries, the putrefactive matter in water serving for the nourishment of aquatic plants, v. 62. Phlogisticated air is somewhat mended by agitation in yellow nitrous acid, iii. 128. It is not easy to convey it to any great distance in the same state, iv. 270.

Several insects will live very well in air tainted with putrefaction, though it is fatal to all animals that breathe it, i. 86.

P A R T IV.

Facts relating to FIXED air.

Fixed air is not yielded by pit coal, though the ashes of it contain a great quantity of this air, iv. 393, but Bovey coal does contain fixed air, ib.

Fixed air is contained in saline substances, ii. 115, in vitriolated tartar and Glauber salts, v. 165, in alum, ib. It is extracted from the calces of metals by heat, ii. 111, and from clay, ii. 215. A great quantity of it, mixed with inflammable air, is contained in cream of tartar, iv. 403. It is retained obstinately by cream of tartar exposed to heat, iv. 405. It is extracted from the earth that Mr. Godfrey obtained from water, *Philosophical Empiricism, Advertisement.*

No vitriolic acid contained in fixed air obtained by the means of it, shewn by Mr. Hey, i. 288. Shewn by Mr. Bewly not to partake of the nature of the acid by which it is procured from calcarious substances, ii. 382. Proved by him to be a particular acid, 337.

A solu-

A solution of mercury in spirit of nitre yields fixed air in consequence of being exposed to the atmosphere, iii. 352, iv. 388. Wood ashes imbibe fixed air from the atmosphere, iii. 353, iv. 390; so also do pitcoal ashes, iv. 392; but it is not attracted by bone ashes, 394. It is procured from pitcoal ashes repeatedly after being mixed with nitrous acid, iii. Preface 33. It is also generated repeatedly from wood ashes and spirit of nitre, iii. 31, &c. and from minium with spirit of nitre, 35. Fixed air is procured from spirit of wine and spirit of nitre, 350; as also from vitriolic acid and spirit of wine, iv. 384, and from vitriolic acid and ether, 386.

The residuum of fixed air is the same thing with phlogisticated common air, ii. 331. It has a residuum not imbibed by water after being expelled from water, 219.

A great quantity of fixed air is procured from mice putrefying in water, iii. 340.

It seems to be deposited from common air by burning inflammable substances in it i. 44, but not when brimstone is burned,
45. It

45. It is found in common air restored by agitation in water, and then phlogistified by nitrous air, ii. 218, and in all cases in which dephlogistified air is procured, even from precipitate per se 217. Less fixed air is discovered in common air when it is phlogistified by respiration, than by putrefaction v. 111, 118. None is found when it is phlogistified by the firing of inflammable air 124.

Fixed air contained in water is easily imparted to any kind of air that is transmitted through it, iii. 355. It is discharged from water by removing the pressure of the atmosphere i. 34. It is not imbibed by ice, 33. It does not of itself dissolve iron, 215.

One measure of fixed air saturates almost three measures of alkaline air, iii. 293. It changes red rose leaves white i. 36. iii. 316. It is fatal to vegetables i. 36. iii. 308, but not soon fatal to insects, i. 36.

Water impregnated with fixed air is fatal to vegetables which have their leaves in it iii, 321. It kills the plants which have their roots in it, iv. 329. It kills fishes, ii. 231.
It

It prevents the putrefaction of flesh meat, observed by Sir William Lee, iv. 461.

Fixed air becomes phlogisticated air by the electric spark taken in it, i. 248, 2.

A saline substance is formed by fixed air with the earth of alum, iv. 445.

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P A R T V.

Facts relating to INFLAMMABLE air.

Inflammable air discovered in the bottom of standing water, related by Dr. Franklin, i. 321.

It is procured from regulus of antimony in marine acid, iii. 255, from metals by the vegetable acid, 256, from cream of tartar, together with fixed air, iv. 403, more of it from steel than from iron, iii. 166, from copper by the marine acid, i. 144, from lead by the same acid, 145, from iron by heat, ii. 107, from iron filings and brimstone in a warm place, iii. 258, and also in time in the common temperature of the atmosphere, v. 83, from zinc and brimstone in a warm place, iii. 259, from a solution of galls with iron filings, iv. 361.

Inflammable air is procured by taking the electric spark in oil, i. 244, in spirit of wine, 245, and in volatile sal ammoniac, ib.

The

The electric spark taken in alkaline air produces three times the quantity of inflammable air, v. 218.

A considerable quantity of it was extracted from oil of turpentine, iv. 363. It was increased by agitation in oil of turpentine, iii. 266, and also in spirit of wine, but not in the same degree, ib. After this, its inflammability was much lessened, ib.

A species of temporary inflammable air was made by Dr. Ingenhoufz from ether, iv. 474.

Inflammable air contains the same quantity of phlogiston with nitrous air, bulk for bulk, iv. 378. It contains no acid, 364, 377. Water impregnated with it does not turn the juice of turnsole red, iii. 268.

Inflammable air recently made has a smell according to the substance from which it is extracted, i. 57.

It is not affected by the electric spark, iv. 367. The colour of an electric spark taken in it is red, i. 61. It is fired by Mr. Volta with an electric spark, iii. 382.

Inflammable air, by long standing in water, is much diminished in bulk, and becomes phlogisticated

phlogisticated air, i. 59. This process is accelerated when the water has been boiled, 67, and by agitation, 68.

Inflammable air mixed with the fumes of spirit of nitre, is fired at one explosion, i. 65. When it is agitated in pale spirit of nitre the quantity of it is increased, and it is fired with a still greater explosion, iii. 262. If the nitrous vapour continue long in it, the vapour is reimbibed by the acid, and the inflammable air is fired as usual, 264.

Inflammable air mixed with fixed air burns with a blue flame, ii. 110. With nitrous air it burns with a green flame, i. 117.

When inflammable air is made respirable by agitation in water, and is then phlogisticated by nitrous air, it does not make lime water turbid, i. 188.

Inflammable air is diminished by florid blood, iii. 76. It is decomposed by flint glass, in a red heat, making the glass black, iv. 368. The transparency of the glass is restored by heating minium in it, 376. Inflammable air is imbibed by water, and expelled again by heat, in the same state, iii. 267. When imbibed by charcoal, it comes

out less inflammable, but that which is not imbibed is as inflammable as ever, iv. 378, It is imbibed by the willow plant, 322.

Common air is phlogisticated by decomposing inflammable air in its nascent state, v. 92.

When animal substances putrefy, all the inflammable air they yield is extricated before the whole of the fixed air, iii. 343, v. 79.

Alimentary substances yield inflammable air by putrefaction, v. 65; but do not part with it in boiling, 74.

Inflammable air is fatal to animals, i. 62, benumbs wasps and other insects, but does not kill them, 247.

Its refractive power is greater than that of common air, observed by Mr. Warltire, iii. 365.

P A R T VI.

Facts relating to NITROUS air.

Nitrous air is procured by dissolving several metals in spirit of nitre, i. 110, from lead, ii. 174. Twice as much is got from mercury
after

after it is completely dissolved in spirit of nitre, than during the solution, iv. 266. Only one third of the quantity of dephlogistified air is procured from the same solution, ib. Some remarkable phænomena attend the solution of iron in nitrous acid with respect to the production of air, iii. 169.

The quantity of nitrous air does not depend upon the quantity of water in which the same metal is dissolved, iii. 168. But the quantity of it is nearly in proportion to the quantity of water with which the acid is usually diluted, in order to dissolve any particular metal, ii. 225. But though water may possibly enter into the composition of it, v. 171, there is no water discovered in the decomposition of it, 172. Almost three times as much is procured from iron, as from copper, iii. 166. It is procured from liquid substances containing phlogiston, ii. 124, from gums, &c. ii. 125, from charcoal, 137. It is yielded by vegetable substances more than by animal ones, 142. Of animal substances it is most readily yielded by fat, and the brain, 157.

Nitrous air is produced by impregnating distilled water with nitrous vapour, iii. 199, and also with a pure vapour of nitrous acid, without the solution of any metal, iv. 66.

Nitrous air diminishes common air about one fifth, and itself wholly disappears, i. 110. It diminishes no air but respirable air, 114.

Nitrous air is not changed by keeping two years in a phial close corked, iii. 358. It is not changed by being exposed to heat in a flint glass tube hermetically sealed, iv. 46. Nor when confined with water in the same circumstances, v. 177. Nor by expansion with heat over quicksilver, and mixed with water, iv. 46.

Nitrous air is not heavier than common air, i. 119.

The nitrous acid that enters into the copper is six times as much as enters into the nitrous air produced by the solution of it, iii. 162.

Nitrous air is imbibed by water, and is expelled again by heat, without any change of property, iii. 109. It is also expelled from water by freezing, 359. The residuum that

is

is not imbibed by water is phlogisticated air, i. 120; but after much agitation in water it becomes respirable air, and is diminished by fresh nitrous air, 189, and without forming any incrustation when the process is made in lime water, 190. Water impregnated with nitrous air deposits a sediment when it is frozen, iii. 559.

Water made blue with the juice of turn-sole becomes red by being impregnated with nitrous air, iii. 108. In other respects it is not sensibly acid, till it be decomposed by common air, as observed by Mr. Bewly, i. 318; but by the decomposition of much nitrous air in contact with water a strong nitrous acid may be procured, 320. Four ounces and a half of water will receive the nitrous acid from three hundred ounce measures of nitrous air, when it becomes blue, iii. 161. The acid in this water is extremely volatile, iii. 162.

An impregnation of nitrous air gives a purple colour to vitriolic acid, iii. 129, and a blue colour to spirit of salt, ib. It is also absorbed by radical vinegar, and by water impregnated with vitriolic acid air, 130.

It gives a green colour to a blue solution of copper in spirit of nitre, v, 176.

Nitrous air agitated in nitrous acid is made, in a considerable degree, respirable, iii. 128.

When nitrous air is kept in a bladder, it never diminishes any kind of air without an appearance of fixed air, i, 191, 214. Water impregnated with it sometimes makes a deposit of white matter, iii. 104.

Nitrous air resists putrefaction, i. 123. It preserves animal substances, but not long in a state fit for culinary purposes, iv. 69. Bile impregnated with it is long preserved from putrefaction, 74.

Nitrous air is fatal to plants, i. 119. even to the willow plant, v. 13, and to insects, i. 226.

Nitrous air becomes phlogisticated air when diminished by long keeping in water, iv. 62. When imbibed by charcoal, both the remainder, and that which is expelled from the charcoal by water, is phlogisticated air, iv. 454. It is diminished to one fourth by iron filings and brimstone, i. 118, much more by liver of sulphur, 219. When decomposed

composed by iron filings and brimstone, it imparts no acidity to the water in contact with it, iii. 143, 146.

Nitrous air is decomposed by pyrophorus, iv. 64, by nitrous vapour, ii. 170. It is absorbed in a very great quantity by the nitrous acid, iii. 122. It is diminished by florid blood 76. It is decomposed by a solution of green vitriol, iii. Preface, 33. In this process the solution becomes black, iv. 48, and this is the same whether the nitrous air has been got from iron or copper, 50. The solution recovers its colour by exposure to the air, which it phlogisticates, 52.

Nitrous air is decomposed by olive oil, which is coagulated by it, iv. 75. It is readily absorbed by oil of turpentine, which takes more than ten times its bulk of this air, iii. 112. It is absorbed by ether, 115, by alkaline liquors, 118, by spirit of wine, 119.

Nitrous air is diminished by being kept in a bladder alternately moist and dry, iii. 155. The water in contact with it in this process is very acid, 157.

This air is diminished very much by the electric spark, i. 222. iv. 64.

A great proportion of nitrous air becomes phlogisticated air immiscible in water by long keeping, v. 178.

Facts relating to DEPHLOGISTICATED NITROUS air.

Nitrous air is partially dephlogisticated by long exposure to iron, i. 215, v. 194, 203. This dephlogisticated nitrous air is procured immediately by the solution of tin, iii. 17, 24, by the solution of iron with heat, 133; but in this case it burns more like inflammable air, 134. It is also procured by the solution of zinc, 135. It is produced in great plenty by a solution of copper upon iron, v. 200. It is procured from iron filings and brimstone by nitrous air before it becomes phlogisticated air, iii. 141. It is procured suddenly after a considerable diminution of nitrous air, iv. 56. The slower the process is, the more of the nitrous air will become phlogisticated, 57.

Dephlogisticated

Dephlogisticated nitrous air is procured in great purity by making water imbibe it, and then expelling it again by heat, v. 213. It becomes purer air by being kept united to water, 214.

Alkaline air does not affect this air, v. 216; nor is the colour of juice of turnsole changed by it. When absorbed by water, it imparts no acidity to it, iii. 143, 146.

P A R T VII.

Facts relating to MARINE ACID air.

Marine acid air is procured by heat from spirit of salt, i. 146, and from salt and oil of vitriol, 229.

It consists of the marine acid in a state of vapour, i. 147, and probably contains phlogiston, ii. 5. It is heavier than common air, i. 241. It is converted into a white substance by heat, in a glass tube hermetically sealed, iv. 101.

With alkaline air it forms the common sal ammoniac, i. 170. One measure of this air absorbs 1 1-6th of alkaline air, iii. 294.

Marine

Marine acid air extinguishes a candle with a blue flame, i. 147. It dissolves iron, 149. It dissolves sulphur and nitre, 149. It coagulates oils, 150. It dissolves ice, 140. It makes camphor fluid, 235. It makes blue vitriol green, 237. It dissolves white vitriol, ib. It deprives borax of its water, ii. 238.

It makes inflammable air with many substances containing phlogiston, i. 149, with wood, dry flesh, &c. 231, with quick lime, 238. It also forms a permanent air with liver of sulphur, ii. 233. It is a little diminished by the electric spark, 239.

United with water it forms the marine acid, i. 148, and then appears to be twice as heavy as water, ib. It is imbibed by ether, 234.

P A R T VIII.

Facts relating to VITRIOLIC ACID air.

Vitriolic acid air is procured by heating in oil of vitriol almost any substance that contains phlogiston, ii. 2, &c. but not from gold

gold or platina, 20. It extinguishes a candle without any particular colour, 7. It is heavier than common air, 7, and than alkaline air, 9.

Vitriolic acid air phlogisticates common air, ii. 10. It will not dislodge the nitrous acid, or the marine acid, from any solid substance in which they are contained, 11. It dissolves camphor, 13. It deprives borax of its water, 14.

Sulphur is formed from water impregnated with vitriolic acid air in a long continued heat, iv. 124. White crystals are formed in a glass tube, containing vitriolic acid air exposed to heat, 131.

Vitriolic acid air uniting with alkaline air makes vitriolic ammoniac ii. 9. A yellow substance is produced at the same time, 22; but it becomes white by exposure to the common air, iii. 277. One measure of this air saturates two of alkaline air, 292.

Vitriolic acid air united with water makes volatile vitriolic acid, ii, 7. Water imbibes ten times more marine acid air than the vitriolic, iii. 275; but when fully impregnated with either

ther of them, it will not take any of the other 276. Water impregnated with vitriolic acid air in time dissolves some metals, and yields inflammable air, 273. Alum is formed by earth of alum and water impregnated with this air, iv. 123. Water impregnated with this air freezes without losing its air, iii, 361.

Whale oil imbibes six or eight times its bulk of this air, and becomes red, iii, 278. Olive oil imbibes the same quantity, and becomes first colourless, but afterwards of an orange colour, ib. Oil of turpentine also imbibes this air, and assumes an amber colour, ib.

The electric spark taken in vitriolic acid air, confined by quicksilver, produces a black substance, ii. 239. It is produced by explosions, when much more electric matter than they consist of will not do it, iii. 281. The vitriolic acid air is diminished by this process, 280. This black matter is the same whether the air was produced from copper, quicksilver, or any other substance, 282.

P A R T IX.

Facts relating to FLUOR ACID air.

Fluor acid air is procured by dissolving the fluor in hot oil of vitriol, ii. 190. It is capable of being confined by quicksilver, 189.

Water saturated with this air gives out an air that has all the properties of vitriolic acid air, ii. 207. Water and alkaline air require the same quantity both of vitriolic acid air, and of fluor acid air, to saturate them, iii. 289. Something similar to this air is procured by oil of vitriol and Mr. Canton's phosphorus, ii. 212; but this may perhaps be sulphur formed in the solution and sublimed, iii. 287.

This air extinguishes a candle, ii. 197. It forms a white substance with alkaline air, ib. One measure of this air saturates two measures of alkaline air, iii. 292.

Very little of this air is absorbed by quicklime, or chalk, ii. 200. It is absorbed by charcoal,

charcoal, rust of iron, and alum, 200. It dissolves nitre, 201. Borax becomes soft in it, 204. Fluor acid air confined in a glass tube, and heated, corrodes it very much, iv. 434.

Water admitted to fluor acid air becomes acid, and a white substance, called the fluor crust, is deposited, ii. 190. Water impregnated with this air will not freeze, iii. 361, except with a great degree of cold, iv. 443.

This air is imbibed by spirit of wine, ii. 199, by vitriolic and nitrous ether, ib. and by oil of turpentine, 199, 211.

P A R T X.

Facts relating to ALKALINE air.

Alkaline air is produced by heat from caustic volatile alkali, i. 164, and also from sal ammoniac and flaked lime, 166. It consists of volatile alkali in the form of air, 164.

Alkaline air is heavier than inflammable air, i. 176; but lighter than marine acid air,
ib.

ib. It does not unite with oils, 172. It takes water from alum, 174. It dissolves ice, 176. It will not dissolve copper, ii. 232.

The electric spark taken in alkaline air produces inflammable air, ii. 239, and the quantity of inflammable air is three times that of the alkaline air, v. 218, &c.

Uniting with fixed air, it makes the mild volatile alkali, i. 171; with marine acid air it makes the common fal ammoniac, 170, 205, &c. with water it is the volatile spirit of fal ammoniac, 167.

Of this air there was absorbed

By fluor acid air, 1 19-20th oz. measures.
Vitriolic acid air, 2
Marine acid air, 1 1-6th
Fixed air, - 1 6-7ths, Vol. iii. 294,

P A R T XI,

Facts relating to the NITROUS ACID,

The nitrous acid may be exhibited in the form of air for a short time, without being so much loaded with phlogiston as to form
nitrous

nitrous air, ii. 168. The vapour of this acid is colourless, 172. It is capable of being combined with phlogiston without water, and then confined in glass vessels, iii, 186. This nitrous acid vapour becomes of a deeper vapour by heat, 187. The phlogiston leaves it to join with the air with which it is mixed, 192. Its redness disappears when combined with a little water, 196. Water impregnated with nitrous vapour emits a great quantity of nitrous air, 198; and also when it is previously impregnated with vitriolic air, 222.

Water impregnated with nitrous vapour first becomes blue, and then green, when the production of nitrous air ceases; afterwards it is yellow, iii. 198. Water is increased in bulk one third by this impregnation, 202. The nitrous vapour with which water is impregnated is exceedingly volatile, 203, 204. Nitrous acid thus made contains more phlogiston than the common sort, 205. It makes no deposit when mixed with a solution of silver in the nitrous acid, ib.

Nitrous vapour is imbibed by animal oils, and emitted again by heat in the form of phlogisticated

phlogisticated air, iii. 182. Oils impregnated with nitrous vapour become red, but they are blue while hot, in the course of the process, 208. Nitrous ether is made by impregnating spirit of wine with nitrous vapour, 213. The nitrous vapour seizes upon the water of alum, 229.

Nitrous vapour is imbibed by red lead, which then becomes white, iii. 230. The effect is the same whether the vapour has been produced by the solution of bismuth, or of iron, iv. 36. All metallic calces have a strong affinity with the nitrous acid, and when united with it become white, v. 236, &c. These nitrated calces may be procured by distilling the solutions of these metals, 238. When the acid is expelled from them by heat, they will attract it again, 241.

Solutions of copper and mercury in nitrous acid, in a confined and long continued heat, from saline substances that are not deliquescent, iv. 489. A solution of iron is much sooner converted into such a salt, v. 234.

Nitrous vapour is imbibed by oil of vitriol, and separated from it by water, iii. 218. Oil

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of vitriol faturated with it is cryftallized with it, iv. 26, 450. All the liquid in the compofition is then pure fpirit of nitre, 33. If phlogifton be contained in the oil of vitriol, it paffes into this nitrous acid, 452.

Nitrous vapour converts fpirit of falt into the beft aqua regia, iii. 219. It makes various changes in feveral fluid fubftances, iv. 38.

Spirit of nitre may be procured almoft colourlefs by a careful diftillation in the common way, iv. 453. From being of a deep orange, it becomes green by long keeping, 453; afterwards of a deep blue, 454. But if it be expofed to the open air, it becomes orange coloured again, ib.

Nitrous acid phlogifticates common air, ii. 163, 167. It has the fame effect when it is quite colourlefs, iv. 25. Heat deepens the colour of this acid, iii. 249. Its colour is univerfally owing either to phlogifton, or heat, iv. 2.

The nitrous acid diffolves aftringent vegetables with great rapidity, iii, 170. The pale acid, in diffolving copper, yields lefs nitrous

trous air at first, and most afterwards; whereas the smoaking spirit yields the most air at first, and makes a hissing noise when mixed with water, 245. This acid gives more nitrous air by the solution of metals, when it has been volatilized, by being heated together with earthy substances, than it does by the solution of the same metals without being volatilized, 44, 50, 51, 357. The same effect is produced when the acid is volatilized by the solution of bismuth, and then imbibed by water, 250, 356.

Nitrous acid is not easily combined with water after being volatilized, iii. 48, 253. It is phlogisticated and volatilized by nitrous air, 126, 441.

A white substance is produced when the nitrous acid is heated a long time in glass tubes hermetically sealed, iv. 21. When this acid is mixed with the vitriolic, and iron is dissolved by them, the first produce is nitrous air, and the second inflammable, iii. 171.

P A R T XII.

Facts relating to the MARINE ACID.

Marine acid has the same affinity with earthy substances, whether combined with water, or in the form of air, iv. 459.

The colour of marine acid is owing to earthy impregnations, iv. 79. A different substance generally gives it a different colour, 86. The colour of it is discharged by the coal of cream of tartar, 109, by liver of sulphur, 111, and by flowers of zinc, 112. It recovers its colour by exposure to the air, if it has been discharged by liver of sulphur, 112, but not if it has been discharged by flowers of zinc, 458.

The marine acid saturated with the rust of iron deposits something when it is hot, iv. 103, as also do other saturated solutions of this acid, ib. An incrustation is made in glass tubes hermetically sealed, containing a saturated solution of common salt, exposed to a continued heat, 106.

The

The marine acid when hot dissolves glass, iv. 93.

The marine acid is dephlogisticated by the calces of lead and manganese, so as to be incapable of yielding any marine acid air that can be confined by mercury, v. 252.

P A R T XIII.

Miscellaneous Facts relating to ACIDS.

Vitriolic acid is coagulated by quicklime, ii. 229. It deposits an earthy matter at the first distillation, but not afterwards, iv. 116.

A mixture of the vitriolic and nitrous acids, though separately coloured, becomes colourless, and a white deposit is always made, iv. 439. The nitrous acid entirely escapes from this mixture, v. 243, but more readily when it is exposed to nitrous air, 246.

The phosphoric acid is not convertible into air, iv. 135, not even with substances containing phlogiston, ib. It yields inflammable air with minium, 136.

P A R T XIV.

Miscellaneous facts relating to AIR,

No air is produced from radical vinegar, even with any substance containing phlogiston, iii. 329; or from smoking spirit of Libavius, 330; none from caustic alkali and any metal, 332; none from spirit of wine, or spirit of wine and camphor, ib. The principle of smell does not seem to be capable of being exhibited in the form of air, ib. No air is procured from concentrated nitrous acid and copper, iv. 441.

More inflammable air is procured from vegetable and animal substances by a quick than by a slow process, i. 56, iii. 255. More dephlogisticated air is also produced in the same manner, 337.

Different kinds of air that have no affinity do not, when mixed together, separate spontaneously, but continue diffused through each other, iii. 301, iv. 432.

The different kinds of air are expanded by the addition of ten degrees of heat according

to Fahrenheit's thermometer, in the following proportion.

Common air	-	-	1.32
Inflammable air	-	-	2.05
Nitrous air	-	-	2.02
Fixed air	-	-	2.20
Marine acid air	-	-	1.33
Dephlogistified air	-	-	2.21
Phlogistified air	-	-	1.65
Vitriolic acid air	-	-	2.37
Fluor acid air	-	-	2.83
Alkaline air	-	-	4.75

Urine phlogistifies dephlogistified air, and decomposes inflammable and nitrous air, v. 132.

PART XV.

Facts relating to MERCURY.

Mercury containing lead, &c. is cleansed by agitation in respirable air, which it phlogistifies.

gisticates, iv, 149. It is converted into a black powder by agitation in water, 159; and in spirit of wine, 161. This black powder is mercury super-phlogisticated, and it becomes running mercury by exposure to the air, which it phlogisticates, 162, &c. When the water is warmed, it imbibes the phlogiston which made the mercury black, 169. Mercury may be exhibited in four different states in succession, beginning with running mercury, and ending with the precipitate per se, by exposure to heat on a glass plate, 175.

Water that has been often used for agitating mercury has a greater effect in that process than water that has not been used for this purpose, iv. 176.

Mercury is presently divided into small globules by agitation in vinegar, iv. 181.

The vapour of mercury in the temperature of the atmosphere easily pervades vitriolic acid air, v. 225, &c. It is superphlogisticated by the electric spark taken in it, ib.

Precipitate per se is formed by long agitation of mercury in a glass vessel close stopped

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ped, iv. 186. The production of it is favoured by dephlogisticated air, v. 152.

P A R T . XVI.

Facts relating to ELECTRICITY.

The conducting power of charcoal is greater in proportion to the heat with which it is made, ii. 245, &c. A substance which has a peculiar conducting power is formed from turpentine, and other vegetable oils, burned with glass in a confined place, 257.

The electric explosion diminishes common air more than the same quantity of electric matter in sparks; the glass tube in which it is taken is covered with a black substance, which is from the mercury, by which the air was confined, 3. Preface, 34.

The electric matter leaves an interrupted circuit and passes into any neighbouring conductor, but leaves it the same instant, v. 258, &c.

The electric explosion may be taken in the hot vapour of water, and of quick-silver, iv. 426.

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An explosion may be transmitted through glass much short of a red heat without breaking it, v, 293.

A thick glass tube burst in a remarkable manner by an electric explosion, iv. 428. A coating of cement favours the bursting of glass jars by electric explosions, v. 287, &c.

P A R T XVII.

Facts relating to a LONG CONTINUED HEAT.

In a long continued heat iron is deposited from a solution of it in water impregnated with fixed air, iv. 413. Mercury and copper are also deposited from a solution of them in spirit of nitre, 414. The saline substances thus formed are not deliquescent, 489. Gold is partly crystalized, and partly deposited, from a solution in aqua regia, 416.

An incrustation is formed in a glass tube containing a solution of nitre, iv. 415. Lime is deposited from lime water, 413. A white substance is deposited from caustic volatile alkali,

alkali, 422. The colour of olive oil is changed by exposure to heat, 419.

P A R T XVIII.

Facts relating to MINERAL SUBSTANCES.

Brimstone affects copper dust in the same manner as it does iron filings, i. 157.

Minium contains no nitrous acid, iii. 45. The colour of minium is the same with that of blood. It also becomes darker by heat, and recovers its light colour by exposure to the air, iv. 429. Minium is capable of yielding pure air so long as it retains its red colour, and no longer; but when deprived of it by spirit of salt it still retains that property, 432.

Bismuth and nickel dissolved in marine acid give a smell of liver of sulphur, i. 161.

The solution of copper in volatile alkali becomes blue by phlogisticating the air contiguous to it, iv. 288. The calx of iron, from being blue, becomes red by parting with its phlogiston to the air, 289. Water
takes

takes phlogiston both from phlogisticated and from inflammable air agitated in it, i. 68. The vapour of water much heated corrodes both glass and iron, iv. 410.

P A R T XIX.

Facts relating to the VEGETABLE SYSTEM.

Growing vegetables purify air phlogisticated by the burning of candles, i. 49, by respiration or putrefaction, &c. 87. iv. 305. They imbibe what would contribute to putrefaction, and yield an offensive smell, v. 43, 57.

The willow plant absorbs air of various kinds, iv. 320; and thrives best in inflammable air, v. 1.

Light is necessary to enable plants to purify air, v. 18. But pure air is not *produced* by light or by plants, but only by the purification of the impure air to which the plants have access, 24, 27, 29.

Air is strained through the body of a plant, v. 11. Pure air is found in the bladders of
sea

sea weed, iv. 313. Green vegetable matter or *conserua minima* produces pure air in the light, iv. 337, &c. 489. Heat has no such effect, 346, &c.

The seeds of this plant float in the air, v. 34. It does not appear soon in rain water, or distilled water, 35, 36. It does not appear in water till the fixed air it contained be expelled from it, 39.

Potatoes are favourable to the production of this vegetable substance, v. 49. Onions are unfavourable to it, v. 52, so are blood, fat, gall and gravy, 61, 10, so also are fruits, 73.

P A R T XX.

Facts relating to the ANIMAL OECONOMY.

Air is in different states at different times in the bladders of fishes, ii. 230.

Blood has an attraction for phlogiston, and when saturated with it is of a darker colour, iii. 56, 71. It attracts phlogiston through serum and a bladder, iii. 78.

Animal

Animal substances, in putrefying, yield both fixed air and inflammable air, but the inflammable air is exhausted first, iii. 343, v. 79.

The perspiration of the body does not injure air, iv. 275, v. 104.

No air issues from the pores of the skin, v. 100.

Animals die in noxious air suddenly, 71.

Gall yields a great quantity of nitrous air when dissolved in the nitrous acid, iii. Preface 32.

Animal substances are not reduced in their dimensions by coaling, ii. 244.

P A R T XXI.

Miscellaneous facts.

Charcoal is expanded by heat, like metals, ii. 256.

Lime water does not freeze so soon as common water, iv. 444. Lime water impregnated with vitriolic acid air does not freeze soon, ib.

Homberg's

Homburg's pyrophorus proved by Mr. Bewly not to owe its accension to vitriolic acid, but probably to an affinity between that acid which is the constituent part of the atmosphere and the alkali in that composition, iii. 395. A purely alkaline pyrophorus discovered by him, iv. 479.

Bolognian phosphorus shewn by professor Allamand to give out light of the very same colour with that which it had imbibed, iii. 376.

Sound originates in different kinds of air, and its intensity is nearly in proportion to their density, v. 295.

A volatile alkali is found in the rust of iron produced by the solution of copper in the nitrous acid, v. 301.

SECTION XXXIII.

Experiments and observations made after the preceding sections were sent to the press,

§ 1. *Of the respiration of dephlogisticated air.*

I HAVE observed, p. 163, that I could never make mice live in dephlogisticated air till they had completely phlogisticated it, and I could not, at the time of writing that article, assign any sufficient reason for the fact. Being unwilling to leave this difficulty unsolved, I repeated the experiment; and putting a vigorous mouse into about ten ounce measures of this air, it continued some hours seemingly at its ease, but died while the air was so pure, that, with two equal quantities of nitrous air, the measures of the test were considerably less than 1.0.

I then put another young mouse into the remainder of the air, and it also continued at its ease two or three hours; but then seemed to be expiring, its respiration being very languid,

languid, and so slow, that I several times concluded it was absolutely dead. I did not at first suspect that it could be affected by *cold*, when other mice lived very well in a wire cage in the same room; for it had soon become quite dry after passing through the water, and had never shewn any sign of uneasiness. But bringing it near the fire, when the heat was about 80 or 90 degrees (though I removed it occasionally when it seemed to be uneasy on that account) it lived several hours longer, and when it died the air was as completely phlogisticated as common air is generally found to be when mice have died in it.

This experiment fully satisfied me, that it was nothing in the dephlogisticated air itself that was the reason that mice could not live in it. I observed before, vol. i. p. 9, that a mouse is a tender animal, and after passing through water, requires a considerable degree of warmth; but I did not suspect that it required so much, and such a continuance of it, as I found in this particular case.

§ 2. *Of the quantity of dephlogisticated air that may be procured from nitre.*

The quantity of dephlogisticated air that is yielded by nitre is stated by the Abbé Fontana at a hundred ounce measures from an ounce. I also found the same produce when I used a coated glass retort, and such a heat as that vessel would bear. But from two ounces of nitre, in an earthen retort, which Mr. Wedgwood was so obliging as to make for me of a peculiarly refractory earth, and in an intense white heat, raised by such a furnace as Dr. Black has constructed, I got five hundred ounce measures of air, the whole considerably dephlogisticated, and with very little fixed air. The first produce was so pure, that with two equal quantities of nitrous air, the measures of the test were 0.7; but the last part of the produce came slowly, and the measures were 1.3, which shews that the air was considerably injured by something in the retort. For the air procured by this process
in

in glass is very pure, the measures of the test being generally less than 0.5.

What remained in the retort was a dark green or blue substance, extremely acrid to taste, and deliquescent, weighing, when it was taken out of the retort, about 18 dwts. The air would have weighed about 13 dwts. and the water of crystallization which is 18 parts in 110 of the water, 6 dwts. and a half, so that three dwts. remain to be accounted for of the two ounces. This last was, in part, an *acid vapour* diffused through the air, and not incorporated with it; for whenever I emptied any of the vessels of this air I perceived a very pungent smell. Part of the loss also was the *white cloud* with which the air was often filled when it was produced.

§ 3. Of dephlogisticated nitrous air.

That this species of air is really a dephlogisticated nitrous air, and not a phlogisticated air, as I had originally supposed is more evident from its being produced from nitrous

air by the *scales of iron*, which fly off when it is hammered, and which are iron partly reduced to a calx. I filled a phial with these scales, and then filling it up with mercury, dislodged the mercury with nitrous air. In this state the phial continued near three weeks; when I found the air diminished, I did not note how much, but a candle burned in the remainder just as if the nitrous air had been exposed to iron. These scales wanting much phlogiston to make them iron, must be in a state more ready to receive than to give phlogiston to nitrous air.

Since I wrote the section relating to this species of air, I have produced it much more readily than in the method there described, viz. by applying *heat* to the vessel in which it is produced. But I shall first observe that, immediately after producing a quantity of this air, in the manner directed, p. 200, I filled the vessel up with water instead of the solution of copper; and, in about two days, the vessel (which was a phial, containing near a quart) produced about three ounce measures

measures of air, and was phlogisticated air, extinguishing a candle.

Then, pouring out the water, I filled it again with the solution of copper, and put it into a pan of water, which I heated till it boiled; when, by means of a cork and bent glass tube, &c. I received from it about a quart of air, the first part of which was phlogisticated (owing perhaps to its having been in a state of yielding that air immediately before this process) but afterwards it was a proper dephlogisticated nitrous air, admitting a candle to burn in it quite naturally.

In order to ascertain more exactly the degree of purity to which I could bring this air, I impregnated a quantity of snow water with it, and expelling it again by heat, I found that only one sixth of what had been expelled from the water would not be imbibed by water again; so that, in this method, it may be procured in a state of considerable purity.

Having a quantity of air in this state, and having found that a candle burned very well in it, I put a mouse into it; but it would have

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died

died very soon if I had not withdrawn it. This was on the 17th of March 1781. But on the 21st of the same month, I put another mouse into the very same air, and was surprized to find that it continued perfectly at its ease five minutes. To be quite sure with respect to the other properties of this air, I withdrew the mouse while it was quite vigorous, and found that a candle burned very well in the air, but it was not in the least affected by nitrous air. In this very singular case, nitrous air fails to be a test of the respirability of air. The air made use of in this experiment had been kept in a cup of mercury, but some water was in the vessel along with it, and this water had imbibed a little of the air.

I then expelled more of this air, from some water that had been impregnated with it on the 17th day of March, but a mouse died it, and almost as soon as it would have done in any other kind of noxious air. These facts afford more matter for speculation; but I shall make a few more experiments before I give myself much scope in that way.

§ 4. *Of a solution of copper in volatile alkali exposed to heat.*

I have observed that a non deliquescent saline substance is deposited from solutions of copper, mercury, and iron in the nitrous acid, exposed to a continued heat. I have likewise found a similar result with respect to a solution of copper in volatile alkali. In one day a similar deposit was made from this solution in the same circumstances. The substance deposited was of a dark blue colour. and adhered firmly to the glass; and when the vessel was first opened, there was a pretty strong smell of volatile alkali.

§ 5. *Of the power of the different kinds of air to conduct heat.*

One of the first things that I proposed to try, with respect to the different kinds of air, was to observe their *power of conducting heat,*

and I had a contrivance for that purpose when I was at Leeds. The thing, however, appearing to me to be of less consequence than other things that I had in view I deferred it till very lately; when, in consequence of the doctrine of heat becoming, by means of Dr. Crawford's book, the subject of general conversation, I determined to execute what I had so long projected.

For this purpose I prepared the vessel represented fig. 2. and described in the *Introduction*. The thermometer was a very sensible one, and the scale large, so that I could mark upon it twenty divisions, each larger than half an inch between the mean temperature of the atmosphere, and a heat much below that of boiling water. After several trials I at length adjusted it in such a manner, that, having filled the vessel with any kind of air, I could plunge it to a certain depth, first in *hot*, and then in *cold* water, so that the mercury would rise to the division 20, and fall to that of 6 or 7 in a reasonable time. I had a clock that beat seconds close by me, and was so situated, that I could not well
make

make a mistake of more than two seconds, in noting the time when the mercury came to any particular division. The precautions I used to plunge the vessel the *same depth* in the water, in all the experiments, and to exclude all other differences, except what might be occasioned by the different kinds of air, it would be tedious to recount; and no person conversant in experiments, and who is disposed to repeat them, will need to be so minutely instructed.

I will just observe, however, that in the vessel I used for *hot* water, I always made it boil, and it was so full, that the plunging of my air vessel into it made it run over; and the vessel for cold water was always fresh brought from the same pump. The mouth of the air vessel was in a cup of mercury, always filled to the same height; and by this means I could try in the same manner even those kinds of air, that could not be confined by water.

The best account that I could give of the result of these experiments would be to exhibit them in the form of *tables*, of the time at which the mercury reached all the degrees
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of the scale, both in ascending and descending; which tables I have drawn up. But this I shall defer till I have an opportunity of repeating all the observations. At present I would only observe, that all the differences were not so striking as I expected to have found them, but that *inflammable air* conducted heat much better than any other kind of air, the mercury ascending the same space in about half the time that it took up in common air. *Fixed air*, and all the kinds of *acid air* conducted heat considerably worse than common air. *Alkaline air* conducted heat rather better than the acid airs, and dephlogisticated air a little worse than common air, but so little, that I would not answer for the same result in repeating the experiment.

N. B. In the course of these experiments, I could not avoid observing so great an expansion of *alkaline air* by heat, that I conclude the observation in vol. iii. p. 347, may be perfectly accurate, though the extraordinary nature of it made me entertain the doubt I have there expressed concerning it.

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A P P E N D I X.

N U M B E R I.

*Extract of a Letter from MR. ARDEN, Lecturer in
Natural Philosophy, dated September 25, 1772.*

ABOUT fourteen or fifteen years ago, in the presence of William Constable, Esq; at his seat at Burton Constable, in Holderness, I made the following experiments.

I placed a large coated jar that would hold three or four gallons, directly under the prime conductor of a very good electrical machine. The prime conductor was at least eight, or ten inches above the top of the jar, and the communication was made by a brass wire bent at one end over the prime conductor, and the other end passed through a small glass tube (contrived by Mr. Constable to prevent the electric matter from easily flying off) was suspended in the middle of the jar, and had a small piece of brass chain fastened to it, that rested on the bottom of the jar.

I then began to turn the wheel, and after turning about 100, or 150 times, as low in the jar as I could see for the coating, I perceived a ball of
fire,

fire, much resembling a red hot iron bullet, and full three quarters of an inch in diameter, turning round upon its axis, and ascending up the glass tube that contained the brass wire, which was the conductor to the inside of the jar.

I immediately asked Mr. Constable if he saw the ball of fire? He said, Certainly. I said, I will turn on. He answered, By all means. I kept turning the wheel, and the ball of fire continued turning upon its axis, and ascending up the glass tube till it got quite upon the top of the prime conductor. There it turned upon its axis some little time, and then gradually descended, turning upon its axis as it had done in its ascent, and so continued till it was so much below the top of the coating that we could no longer see it. But soon after this, a very great flash was seen; a large explosion was heard, and strong smell of sulphur was perceived all over the room, a round aperture was cut through the side of the jar, as fine as if it had been cut with a diamond, rather more than three quarters of an inch in diameter, and between two and three inches below the top of the coating, and the coating was torn off all round the aperture, about three or four inches in diameter. The jar was a pretty strong one, of crown glass.

I then took another jar, so like the first, that when both were whole I could not easily perceive any difference between them. I then attempted to charge this jar, in the same manner as the other,
and

and we both observed it very accurately. No ball of fire was seen, but presently the jar discharged itself with a great flash, and explosion, and at about the same part as of the first jar, but instead of the aperture which was made in the first jar, there was a circle about three quarters of an inch diameter, as white as chalk, and the coating torn off round about it as before. Upon touching the white part, it dropped out; and appeared to be glass in a fine powder.

We broke several other different sized jars that day (which made Mr. Constable say we were in great luck) but without any thing else remarkable.

The first experiment was made soon in the afternoon of a clear day, and the machine stood directly between us and a window, which was not above a yard from it. I don't hear that this ball of fire has been produced by art by any one else to this day, although it is often seen produced by nature.

I had the pleasure of seeing Mr. Constable this day, and of reading the account of these experiments to him, and to the best of his memory, he thought the whole was strictly true.

Mr. Constable thinks it would not be difficult to repeat the experiment, and to produce the ball of fire at any time, provided the jar is large, and not coated too near the top, and that the wire communicating from the prime conductor, to the
inside

inside of the jar, is made to pass through a small glass tube (which is certainly of great advantage in making experiments of this kind) and that the machine acts very strong. If not, it will be in vain to attempt it.

The fact mentioned in the preceding letter is of a very remarkable nature, and being perfectly well ascertained, it is of importance that it be generally known, and kept in view. For though no person that has hitherto been made acquainted with it has been able to repeat the experiment, others may be more fortunate. Dr. Franklin, and, if I mistake not, Mr. Canton also, and myself, were present when Mr. Henley endeavoured to produce this appearance; but though every expedient that any of us could suggest was made use of, we had no success, and I have several times attempted it in vain since. I shall not, however, desist from my attempts.

Mr. Arden's own evidence is abundantly sufficient to authenticate the fact, and I have since had from Mr. Constable himself the same account of it. Could we repeat this experiment, there would not, I think, be any natural phænomenon in which the electric fluid is concerned, that we could

could not imitate at pleasure. This circumstance alone makes it a very interesting object of investigation.

NUMBER II.

*Extract of a Letter from MR. BEWLY, containing
Observations on some Parts of this Volume.*

PAGE 115, &c. The following observations on this subject may perhaps deserve your attention. I had long ago observed, that on breathing through an infusion of litmus, the same change of colour was produced as when it was exposed to the action of fixed air, or other acids. I put about two ounces of the infusion into two tall cylindrical glasses; one of which stood on the table as a standard, while I breathed through the other. Before the end of the third expiration the latter became red. I then added to it two drops of a saturated solution of fixed alkali; which restored to the infusion its blue colour. After three deep expirations, however, the liquor became again red. I then added to it ten drops more of the *lixivium tartari*; and after about 35 or 40 expirations, the liquor was again changed red;

red; or, to use the language of my former letters on fixed air, the alkaline liquor was not only neutralized, but super-saturated, or acidulated, with the *mephitic acid*, as I suppose this to be: the red colour, thus given to the infusion, flying off when the liquor was exposed to the air; in the same manner as when it has been impregnated with fixed air.

These trials seem to prove that the quantity of fixed air, or at least of a certain volatile acid, emitted in breathing is not inconsiderable: as in three expirations enough was detained in the liquor to more than neutralise two drops of *lixivium tartari*. By a different mode of experimenting (in close vessels) perhaps the quantity of fixed air precipitated from the common air inspired into the lungs might be ascertained with some degree of accuracy.

The result of the following experiment was unexpected. I breathed through two ounces of pure water ten or twelve times; expecting that, on immediately pouring into it a small quantity of a strong infusion of litmus, the liquor would become red: but no change of colour was observed; and three expirations were requisite, as before, to produce a red colour. This seems to shew that water alone will not separate fixed air from the atmospherical air that has been expired from the lungs; but that the litmus performs that office.

office. An accident has prevented me from prosecuting these experiments; and particularly from trying whether an alkaline salt dissolved in *pure* water would be neutralised by the air expired from the lungs.

P. 218, &c. Is it necessary to suppose that the electric matter itself furnishes the phlogiston to the inflammable air, into which alkaline air appears to be converted? Volatile alkali in itself contains a very large portion of inflammable substance. Thus nitrous ammoniac, or *nitrum flammans*, that is, volatile alkali neutralised with spirit of nitre, deflagrates without any addition of inflammable matter, or by means of heat alone. The electric fluid may possibly add to the quantity of inflammable matter contained in the alkaline air, by carrying phlogiston with it from the bodies that *conduct* it: but I think it's principal and most striking effect, in this curious experiment, is it's withdrawing the volatile alkali from our cognisance; possibly by causing it to form a combination with some other principle, which renders it insoluble in water.

But may not the electric spark, in this case, act merely by it's *heat*; and might not dry alkaline air, confined in a tube made red hot, undergo some such change? I do not remember that you ever tried *dry* alkaline air, in your experiments with tubes in a sand heat; but in your fourth volume, caustic *spirit of sal ammoniac* (as
C c it

it ought to have been printed, page 422, line 7 from the bottom) presented an increase of elastic matter; though it had been exposed to little more than a boiling heat.

P. 225. I long ago observed, with no small degree of surprise, the evaporation and condensation of mercury at the top of a barometer in my possession, which terminates in a ball; nor can I now account for some of the singular phænomena presented by it. It is placed on the south side of a window facing the west; and at the distance of nine feet from the fire. On inclining the tube, so as to fill the bulb, and then erecting it; I could, within twenty-four hours, with a strong magnifier, perceive some hundreds of globules condensed on that side of the bulb which faced the window or the light. These globules increased daily in number and size; so as, in a few days, to be visible with the naked eye; and at last to fall down, and be succeeded by others. These appearances have presented themselves at all times of the year; nor does a single globule appear on any other part of the bulb.

But the most singular circumstance is that, on turning the tube half round, fresh globules would appear on the side now facing the window; while those now turned to the opposite side would gradually diminish in bulk and number, and at length totally disappear. This disappearance cannot reasonably be ascribed to the superior heat of that
side

side of the bulb which now faced the fire place: as the same events have constantly taken place, when there was no fire in the room. I cannot plausibly account for these appearances, by the mere influence of heat and cold, on the opposite sides of a bulb not one inch and a quarter in diameter; in which the side facing the window, on which the globules were *condensed*, in the *hot months*, must necessarily be much warmer than the opposite side facing the fire, in the *winter*; from which nevertheless the globules would evaporate and disappear.

P. 234, &c. In a late publication, M. de Wafferberg (*Institut. Chem.* Tom. iii) ascribes a somewhat similar property to the nitrous acid when combined with *bismuth*. He affirms that, whatever methods he used to produce a perfectly saturated solution of this semi-metal in spirit of nitre, he could not succeed. On adding more bismuth to a solution of this kind, a copious precipitate falls to the bottom; and the solution still continues strongly acid.

NUMBER III.

Observations on this Volume, with which I was favoured by MR. WATT.

PAGE 149. I suppose the white powder to be lead, sublimed by the help of the acid, and which would not be dissolved by spirit of salt, which forms with lead an insoluble salt, called *plumbum corneum*. The orange colour was from some phlogistic matter the lead had attracted.

P. 226. Vitriolic acid air, being a volatile sulphur, ought to dissolve and volatilize mercury, and would, on receiving more phlogiston, or perhaps by heat alone, become an ethiops mineral.

P. 248. The cause of the great volatility of the mixture of vitriolic and nitrous acid is, that the former has a much more powerful attraction for water than the latter, and by depriving it of the water leaves it in an incoercible state. It should be added, that the nitrous acid having the strongest attraction for phlogiston, takes it from the vitriolic acid, and thereby increases its own volatility.

P. 255. Was not this powdery substance *corrosive sublimate*? Perhaps this is an easier way than the common one of making that preparation.

P. 298. The steam of water is also a vehicle of sound.

NUMBER

NUMBER IV.

A Letter from DR. WITHERING, containing an Account of a new Method of impregnating Water with Fixed Air, illustrated with a Drawing. Fig. 3.

Birmingham, 12th February, 1781.

Dear Sir,

I Have at length finished the apparatus for impregnating water with fixable air which I mentioned to you some time ago ; and can now assert from experience, that it is attended with every advantage that I expected. The inclosed drawing will sufficiently point out to you the different parts of the apparatus, with their uses ; but for the sake of those who are less conversant in these subjects, I have subjoined a particular explanation.

I am, &c. W. WITHERING.

- A. A glass vessel, about 10 inches high in the cylindrical part, and $6\frac{1}{2}$ inches diameter.
- B. A glass vessel, about 12 inches high in the conical part, one inch and a half in the neck, and 5 inches diameter at the bottom.
- C. A copper pipe passing through the stopper of the vessel B, and tied fast into the flexible tube D,

C c 3

D. A

- D. A flexible air tight tube made of strong leather, and kept hollow by means of a spiral wire passing through its whole length.
- E. A conical brass pipe, with a stop-cock fastened to the tube D.
- F. A conical pipe, with a stop-cock G ; into this pipe the end of the tube E is accurately ground so as to be air tight.
- G. The stop-cock cutting off all communication with the atmosphere when the pipe E is removed.
- H.H. Two large hogs bladders, each of which ought to hold two quarts.
- I. A stop-cock to prevent the water rising into the bladders when the vessel A is agitated.
- K. A bladder, tied to the crooked tube with the stop-cock,
- L; which occasionally opens or shuts the communication with the vessel B.
- M. A glass funnel, accurately fitted with the glass stopper N.
- O. The aperture fitted with a glass-stopper, from which the impregnated water is to be drawn for use. Or instead of the glass-stopper, a silver-cock might be more conveniently applied.
- P. The tube opening into the vessel A.

USES.

U S E S.

This apparatus is contrived with a view to the impregnation of water with fixable air, and with all the other ingredients which are found to exist in the most celebrated mineral waters.

In order to effect these purposes, 1st, fill the vessel A with pure water, and add the other ingredients, when such are required; and in the due proportion to the quantity of water, which will be about five quarts.

2^d, Put into the vessel B, as much marble or whiting, in small lumps as will cover its bottom to the height of about two inches. Then pour in water to the height represented by the dotted line.

3^d, See that all the bladders are tied round their respective tubes, so as to be perfectly air tight.

4th, Let the mouth of the vessel A be well fitted with a cork; through a hole in this cork pass the tube P, and upon the cork put melted sealing-wax of the softest kind, or else modelling wax, so as to make the whole air-tight.

5th. The Mouth of the vessel B must be stopped with a piece of mahogany prepared in the following manner. Let the wood be turned in a lathe in a conical figure, but a little larger than what the mouth of the glass will admit. Put this piece of wood into melted bees wax, and heat the wax until the wood begins to grow black.

black. When cool, turn it again in a lathe until it fits the mouth of the vessel. The tubes C, L and M are fitted into holes bored through the wooden stopper, previous to its being immersed in the melted bees wax.

6th, Push the tubes C, L, M, through their respective holes in the wooden stopper; press this stopper into the orifice of the vessel B, and cement the whole with sealing, or modelling wax.

7th, Shut the stop-cocks I and L; having previously pressed the air out of the bladder K; open the stop cocks G and E; then squeeze the air out of the bladders H H, and afterwards press the conical pipe E into the pipe F.

8th, Pour about a large spoonful of the acid of vitriol, known by the name of oil of vitriol, through the funnel M, and stop it with its stopper N. The fixable air now let loose by the effervescence in the vessel B, rising through the tube C, passes into the bladders H H, and distends them.

9th, When these bladders are distended, open the stop cock I, and from the aperture at O, draw out about a quart of water. The space formerly occupied by the water so drawn out will now be filled with fixable air, which presently begins to be absorbed by the remaining water, and is still supplied from the bladders H H, and from the effervescing mixture in the vessel

vessel B. Whenever these bladders are considerably collapsed, more vitriolic acid must be added through the funnel M, so that they may be kept constantly and pretty fully distended.

10th, If an impregnation is speedily required, turn the stop cocks at G and E, and open that at L. Then separate the pipe E from the tube F, and agitate the vessel A. During this time the fixable air that is produced, passes into the bladder K; from which it may afterwards be pressed into the other bladders when the two parts of the apparatus are again united.

11th, During the agitation, close the stop-cock at I, opening it only occasionally to replace from the bladders H H the fixable air absorbed by the water.

12th, If a strong impregnation is required, this process should be carried on in a room, the heat of which does not exceed 48° of Fahrenheit's thermometer.

Reasons for preferring this Apparatus to that in common use.

1st, It can be made at less expence.

2d, Should any part be broken, the repairs are much more practicable, and less expensive.

3d, The whole quantity of fixable air produced is converted to use, and consequently there is no waste of the vitriolic acid.

4th, It

4th, It impregnates three times the quantity of water at one time, more completely, and with less trouble.

5th, The water so impregnated for ever remains so, if the joints and cocks are made perfectly air-tight; and it may be drawn off at different times without injuring the remainder.

Lastly, it may be necessary to observe that the impregnated water receives no taste from the bladders; and that if the vessel A with its water so impregnated be separated from the vessel B, at the conical parting E, F; it may be inclosed in a pyramidical mahogany case, out of the lower part of which the silver cock at O projects, and thus serve for an ornamental as well as a luxurious and salubrious addition to our sideboards, particularly in the summer and autumnal seasons.

N. B. In order that the cocks may continue perfectly air-tight, it will be necessary to supply them about once a year with a very small quantity of unsalted lard.

Modelling wax may be had at the engravers, or it may be made thus. To half a pound of melted bees wax add two ounces of tallow and one ounce of Venice turpentine. Red lead or Spanish brown may be added in quantity sufficient to give it a colour. Continue stirring the mixture until it is cold.

NUMBER V.

A Letter from Mr. JOHN WARLTIRE, Lecturer in Natural Philosophy, on the Firing of Inflammable Air in close vessels.

Birmingham, 18th April, 1781.

SIR,

I had long entertained an opinion that it might be determined whether heat is heavy or not, by firing inflammable air mixed with common air, and applying them to a nice balance; but as I conceived the danger of passing the electric spark through so combustible a mixture in a close vessel to be greater than it is, I was deterred from making the experiment; 'till, being encouraged by you, I procured a copper ball, or flask, which holds three wine pints, the weight 14 oz. with a screw stopper adapted to it, and began with small quantities of inflammable and large quantities of common air, which were fired without the least danger.

I then increased the bulk of the inflammable air to half that of the common air, which when fired made the flask very warm to my hand; and every time I applied a long glass tube fastened to the pipe of a pair of bellows, to blow the phlogisticated air out of the flask, I observed a smoke escape along with it. I also fired the air when the flask was under water, and did not observe any thing escape when I perceived the heat against my hand with which I kept the ball from rising. When the stopper was unscrewed, the external air always rushed into the vessel containing the phlogisticated air with some violence.

The

The method I usually practice to mix the airs in any proportion, is accurately to fill a measure with inflammable air, and rest it in a tub, with its rim barely under water, hanging over the edge of a shelf, so far as to admit one leg of an inverted syphon, the other leg being closed, but afterwards opened, and the copper flask inverted upon it, but closed with its stopper when the measure of air has been plunged under water, to force it out through the syphon. I have sometimes exhausted the common air to admit the inflammable air into the flask, but I do not find that that circumstance produces any difference in the result of the main experiment.

My next object was to adjust the balance in such a manner as that I could always be certain to weigh to less than a grain when it was loaded with the flask and its counterpoise, and I constantly examined it at the beginning and end of every experiment. The apparatus being adjusted, I proceeded to make the experiment I had in view, and always accurately balanced the flask of common air, then found the difference of weight after the inflammable air was introduced, that I might be certain I had confined the proper proportion of each, the electric spark having passed through them the flask became hot, and was cooled by exposing it to the common air of the room; it was then hung up again to the balance, and a loss of weight was always found, but not constantly the same; upon an average it was about two grains.

I have fired air in *glass* vessels since I saw you venture to do it, and have observed, as you did, that though the glass was clean and dry before,
yet

yet after firing the air, it became dewy, and was lined with a sooty substance.

If you think these experiments worth communicating to your philosophical acquaintance, it may be depended upon that the circumstances appeared to me as I have represented them, whatever they may be found to prove.

I am, with great esteem,

Your humble servant,

JOHN WARLTIRE.

The preceding article, though coming too late to be printed together with the rest of the volume, and to be noticed in the contents of it, I have thought proper to insert on account of the remarkable facts it exhibits.

Dr. Withering and myself were present when the mixture of common air and inflammable air was fired repeatedly in the close copper vessel, and we observed that, notwithstanding all the precautions we could think of, the vessel certainly weighed less after the explosion than it had done before. I do not think, however, that so very bold an opinion, as that of the latent heat of bodies contributing to their weight, should be received without more experiments, and made upon a still larger scale. If it be confirmed, it will no doubt be thought to be a fact of a very remarkable nature, and will do the greatest honour to the sagacity of Mr. Warltire,

I must

I must add, that the moment he saw the *moisture* on the inside of the close glass vessel in which I afterwards fired the inflammable air, he said that it confirmed an opinion he had long entertained, viz. that common air deposits its moisture when it is phlogisticated. With me it was a mere random experiment, made to entertain a few philosophical friends, who had formed themselves into a private society, of which they had done me the honour to make me a member.

After we had fired the mixture of *common* and inflammable air, we did the same with *dephlogisticated* and inflammable air; and though, in this case, the light was much more intense, and the heat much greater, the explosion was not so violent, but that a glass tube about an inch in diameter, and not exceeding one tenth of an inch in thickness, bore it without injury. Nor shall we wonder at this, when we consider that the expansion of air by heat does not go beyond four or five times its bulk. It is evident, however, from this experiment, that little is to be expected from the firing of inflammable air in comparison with the effects of gunpowder; besides that after firing of inflammable air there is a great diminution of the bulk of air, whereas in the firing of gunpowder there is a production of air.

A N

ALPHABETICAL INDEX

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ERRATA.

P. 32, l. 7, for *had*, read *has*.

45, l. 1 (b) for *proportion*, read *portion*.

65, l. 5 (b) dele *a*.

93, l. 1 (b) read *between one half and one third*.

117, l. 15, read *I then used*.

126, l. 6 (b) for *in*, read *into*.

174, l. 2, — *at*, — *it*.

216, l. 9 (b) — *made*, — *made in it*.

239, l. 6 (b) — *of*, — *with*.

251, l. 4 (b) — *vapours*, — *vapour*.

284, l. 3 (b) — *they*, — *they were*.

P. 25, l. 3 (b) Note, for *carried in*, read *carried on in*.

(b) Signifies *from the bottom*.

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